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MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

TECHNOLOGY QUARTERLY

AND

PROCEEDINGS OF THE SOCIETY OF ARTS.

VOLUME V.

109717
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BOSTON:
1892.

The Society of Arts, established in conformity with the plan of the Massachusetts Institute of Technology as set forth in the act of incorporation, held its first meeting on April 8, 1862.

The objects of the Society are to awaken and maintain an active interest in the practical sciences, and to aid generally in their advancement in connection with arts, agriculture, manufactures, and commerce. Regular meetings are held semi-monthly from October to May.

The Society has discontinued the publication of the *Abstracts of Proceedings*, and now publishes its proceedings and the principal papers read at its meetings in the *Technology Quarterly*. The present volume, the first under the new arrangement, contains the proceedings from October, 1891, to December, 1892, inclusive.

The *Quarterly* contains, also, the results of scientific investigations carried on at the Institute, and other papers of interest to its graduates and friends.

Neither the Massachusetts Institute of Technology nor the Society of Arts assumes any responsibility for the opinions or statements in the papers.

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TECHNOLOGY QUARTERLY

AND

PROCEEDINGS OF THE SOCIETY OF ARTS.

VOL. V. APRIL AND JULY, 1892. NOS. 1 AND 2.

PROCEEDINGS OF THE SOCIETY OF ARTS.

THIRTIETH YEAR, 1891–92.

THURSDAY, October 8, 1891.

THE 419th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., President Walker in the chair.

The records of the previous meeting were read and approved. The President then introduced Capt. A. H. Russell, of the Ordnance Department, U. S. A., who read a paper on "The Construction of Modern Breech-Loading Rifled Mortars." The paper was illustrated by several lantern slides, and is published in full in the present number of the *Quarterly*. An interesting discussion followed the reading of the paper, after which the President extended the thanks of the Society to Captain Russell for his valuable paper, and declared the meeting adjourned.

THURSDAY, October 22, 1891.

The 420th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Mr. G. W. Blodgett in the chair.

The records of the previous meeting were read and approved, and the Chairman then introduced Mr. H. C. Spaulding, of the Thomson-Van Depoele Electric Mining Co., who read a paper on "Commercial

Electricity." The paper was illustrated by numerous photographs and is published in full in the present number of the *Quarterly*. The Chairman extended the thanks of the Society to Mr. Spaulding. The meeting was then adjourned.

THURSDAY, November 12, 1891.

The 421st meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Hon. J. A. Dresser in the chair.

The records of the previous meeting were read and approved, and the Chairman then introduced Prof. Cecil H. Peabody, of the Institute, who read a paper on the "Triple-Expansion Experimental Engine in the Engineering Laboratories of the Massachusetts Institute of Technology." The paper was illustrated by several lantern slides, and is published in full in No. 3 of Volume IV of the *Quarterly*. After some discussion the meeting was adjourned.

THURSDAY, December 10, 1891.

The 422d meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Mr. H. M. Howe in the chair.

The records of the previous meeting were read and approved. Mr. Frank B. Howell, of Charlestown, was duly elected an Associate Member of the Society. The Chairman then introduced Prof. William T. Sedgwick, of the Institute, who read a paper on "Milk Supply and the Public Health." The paper is published in full in No. 4 of Volume IV of the *Quarterly*. The meeting was then adjourned.

TUESDAY, December 29, 1891.

The 423d meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Prof. C. R. Cross in the chair.

The records of the previous meeting were read and approved. The Chairman then introduced Mr. Francis R. Hart, of the Massachusetts Electrical Engineering Co., who read a paper on "Long Distance Transmission of Power by Electricity." The paper is published in full in the present number of the *Quarterly*. After a short discussion the meeting was adjourned.

THURSDAY, January 14, 1892.

The 424th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Prof. William T. Sedgwick in the chair.

The records of the previous meeting were read and approved. The Chairman introduced Mr. S. R. Koehler, of the Museum of Fine Arts, Boston, who began a paper on "The Photo-Mechanical Processes." Adjourned.

THURSDAY, January 28, 1892.

The 425th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Mr. Thomas Gaffield in the chair.

The records of the previous meeting were read and approved. Messrs. William Atkinson, of Brookline, and Eliot L. Caldwell, of Boston, were duly elected Associate Members of the Society.

Voted, that the President be authorized to appoint a committee of the Society to confer with a committee of the Corporation to consider the advisability of combining the publications of the Society of Arts and the *Technology Quarterly*. The President subsequently appointed Mr. George O. Carpenter, Col. E. H. Hewins, Professor Lanza and the Secretary to serve on that committee. The Chairman then presented Mr. S. R. Koehler, who continued the reading of his paper on "The Photo-Mechanical Processes." Adjourned.

THURSDAY, February 11, 1892.

The 426th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Prof. William T. Sedgwick in the chair.

The records of the previous meeting were read and approved. The Chairman then introduced Dr. James J. Putnam, of Boston, who read a paper "On Some Recent Discoveries and Problems in the Anatomy and Physiology of the Brain." An abstract of the paper is published in the present number of the *Quarterly*. The paper was illustrated by numerous lantern views, diagrams and models. Some discussion followed the reading of the paper. After the thanks of the Society had been extended to Dr. Putnam, the meeting was adjourned.

THURSDAY, February 25, 1892.

The 427th meeting of the SOCIETY OF ARTS was held in the Walker Building this day at eight P.M., Mr. George Jackson in the chair.

The records of the previous meeting were read and approved. The Chairman then presented Mr. S. R. Koehler, who concluded the reading of his paper on "The Photo-Mechanical Processes." After the thanks of the Society had been extended to Mr. Koehler, the meeting was adjourned. Mr. Koehler's paper will be published in a later number of the *Quarterly*.

THURSDAY, March 10, 1892.

The 428th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., President Walker in the chair.

The records of the previous meeting were read and approved. The President then introduced Mr. T. C. Martin, of New York, who read a paper on "The Hollerith Electric Census System." An abstract of the paper is published in the present number of the *Quarterly*. At the close of the paper the President introduced Mr. Hollerith, of Washington, D. C., who exhibited in operation his machine for tabulating statistics by electricity. On motion of Professor Swain, a vote of thanks was extended to the speakers for their interesting exhibition. The meeting was then adjourned.

THURSDAY, March 24, 1892.

The 429th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Mr. G. W. Blodgett in the chair.

The records of the previous meeting were read and approved. The Chairman then introduced Prof. R. H. Richards, of the Institute, who exhibited and described "A Hand-Telescope for Stadia-Work." At the close of Professor Richards' remarks the Chairman introduced Mr. Harvey S. Chase, of New York, who read a paper on "Magnetic Concentration of Iron Ore." At the close of the paper Mr. Chase exhibited a machine in operation, which was watched with much interest. Both papers are published in full in the present number of the *Quarterly*. The Chairman extended the thanks of the Society to Mr. Chase for his exhibit, and declared the meeting adjourned.

THURSDAY, April 14, 1892.

The 430th meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Mr. H. M. Howe in the chair.

The records of the previous meeting were read and approved. The Chairman then introduced Prof. Jerome Sondericker, of the Institute, who gave "A Description of Some Repeated Stress Experiments." The paper is published in full in the present number of the *Quarterly*. After Professor Sondericker's paper the Chairman introduced Prof. Dwight Porter, of the Institute, who described a "Pitot Tube for Measuring the Velocity of Water in Jets." After a short discussion the meeting was adjourned.

FRIDAY, April 29, 1892.

The 431st meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., President Walker in the chair.

The records of the last meeting were read and approved. Hon. J. A. Dresser, Messrs. J. P. Munroe and George Jackson, and Professors G. F. H. Markoe and William T. Sedgwick were appointed a committee to nominate officers for the ensuing year, to be elected at the next meeting.

Col. E. H. Hewins reported for the committee appointed to confer with a committee of the Corporation on the advisability of combining the publications of the Society and the *Technology Quarterly*, that they recommended the consolidation. Prof. William T. Sedgwick stated for the Faculty of the Institute that they deemed it advisable to unite the two publications, provided that the new publication is conducted by the Society of Arts.

Voted, to accept the report of the Committee.

Voted, to adopt the report, and to refer the matter to the Executive Committee, with power to make the arrangements necessary to carry out the plan.

The President then introduced Prof. C. H. Levermore, of the Institute, who read a paper on "Primitive Customs and Ceremonies of Marriage." The meeting was then adjourned.

THURSDAY, May 12, 1892.

The 432d and annual meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Prof. William T. Sedgwick in the chair.

The records of the previous meeting were read and approved. The Nominating Committee reported the names of the following gentlemen for Executive Committee for the ensuing year: George W. Blodgett, C. J. H. Woodbury, Henry M. Howe, George O. Carpenter, and Charles E. Powers. These gentlemen were then duly elected members of the Executive Committee. Mr. Blodgett reported from the Executive Committee that, to their great regret, the present Secretary declined renomination, and that they recommended Mr. C. W. Andrews for Secretary for the ensuing year. Mr. Andrews was then duly elected. The annual report of the Executive Committee was read, accepted and placed on file. The report of the Meteorological Committee was read, accepted and placed on file, and it was voted that the committee be continued.

The Chairman introduced Dr. H. P. Bowditch, of the Harvard Medical School, who read a paper on "Anthropometry in the Public Schools." The paper was illustrated by the lantern. A short discussion followed the reading. On motion of Mr. Dresser it was voted that the warm thanks of the Society be extended to Dr. Bowditch for his very interesting and instructive paper. The meeting was then adjourned.

THURSDAY, May 26, 1892.

The 433d meeting of the SOCIETY OF ARTS was held at the Institute this day at eight P.M., Mr. G. W. Blodgett in the chair.

The records of the previous meeting were read and approved. The Chairman then introduced Mr. W. J. Salomon, of Newark, N. J., who read a paper on "The Theory and Practice of Tanning." The paper is published in full in the present number of the *Quarterly*. The thanks of the Society were extended to Mr. Salomon, and the meeting was then adjourned.

LINUS FAUNCE, *Secretary.*

THE CONSTRUCTION OF MODERN BREECH-LOADING RIFLED MORTARS.

By Capt. A. H. RUSSELL, U. S. A.

Read October 8, 1891.

The new use of mortars for coast defence was ably discussed before your society, last winter, by Colonel Mansfield, of the Engineer Corps of the Army, and it will be unnecessary to dwell particularly on this point.

We have, however, many important things to consider regarding the manufacture of these engines of war, and this paper will be confined mainly to that subject. Experience with the castings made for thirty mortars at Providence, and a number here, gives, too, some valuable data regarding the Rodman process of casting, and throws some light on the question of using cast iron in the construction.

The modern mortar, shown in longitudinal section in Figure 1, presents little likeness to the apothecary's mortar, which suggested the name for the old type, given in Figure 2, showing on the same scale the old muzzle-loading, cast-iron mortar of 13-inch caliber.

The new mortar has more the size and proportions of a gun, as is seen by Figure 3, giving the outline of a 15-inch Rodman gun, on the same scale. Its right to the name of mortar comes only from its similarity of use for throwing shells high into the air to reach, from above, objects like the deck of a vessel that could not be hit by the direct fire of high-power guns; the latter being effective only against the sides of the vessel. In shape it is a large howitzer, rather than a mortar; but being used like the old mortar for firing at much higher angles than was common with the old shell-cannon called howitzers, it is called in this country a mortar, though often designated a howitzer abroad.

While the old mortar was a short, smooth-bore, muzzle-loading piece, throwing a spherical shell, the new mortar is a breech-loader 129 inches long, nearly as long as the old muzzle-loading Rodman

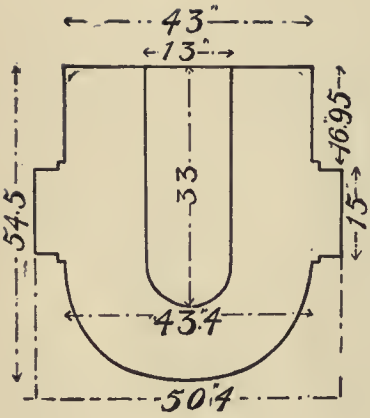


Fig. 2.
Old 13-inch Mortars.

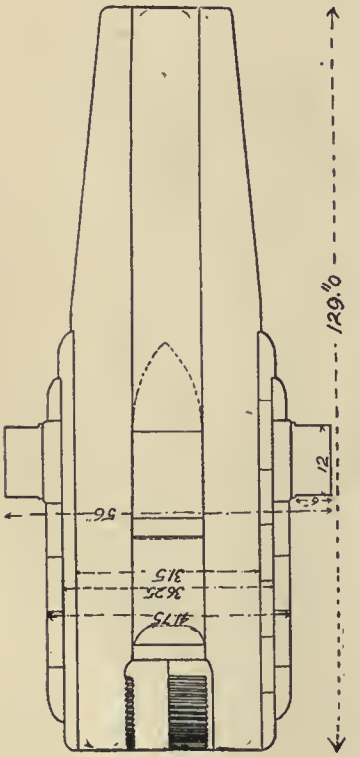


Fig. 1.
Longitudinal Section of 12-inch Breech-Loading Rifled Mortar.

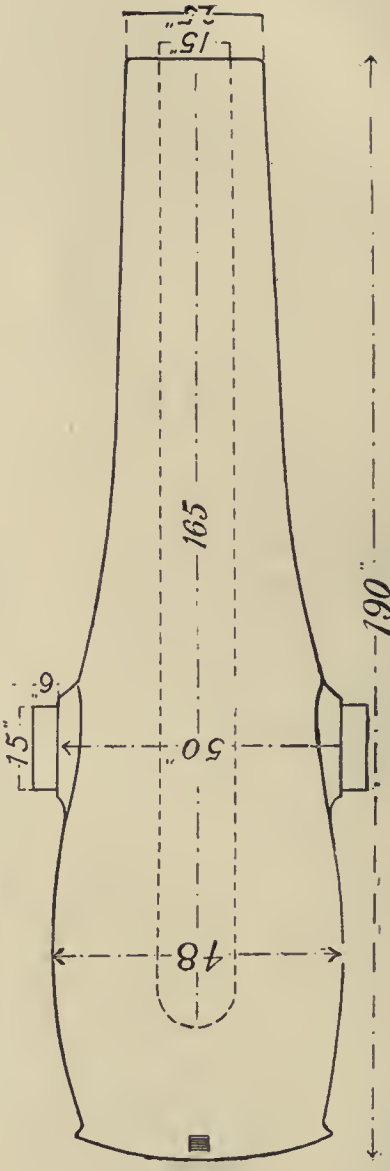


Fig. 3.
15-inch Rodman Gun.

gun, and rifled in the bore to make it possible to throw an elongated projectile (indicated in outline in Figure 1, in the firing position, nearly midway in the piece). The rotary motion given to the projectile by the twist of the rifling is necessary to keep the point of the shell in the direction of flight, much as the spinning of a top is necessary to keep it balanced on its peg. Going point on, the shell encounters less resistance from the air than if moving sidewise, and presents to this resistance a section no greater than the caliber, as with a round ball, while the added weight due to its length gives it greater inertia to maintain its velocity, and causes it to carry further than the round ball. A closer fit in the bore, too, is practicable with the long shell, as a copper band encircles the hard metal of the shell, and wedges into the rifling grooves, securing greater uniformity of motion in leaving the piece, and thus greater accuracy of fire.

The new piece, instead of being fashioned from a single piece of metal, like the Rodman gun, is, like the modern high-power guns, "built up;" *i.e.*, a central tube is encircled by hoops of steel, shrunk on so as to give a strength not practicable with the solid gun. Figure 1 shows these hoops in section. Illustration is given in Figure 4 of the principle on which this construction is based, as drawn from the law first worked out by Barlow for the resistance of hollow cylinders to pressure from within. The inner ring (shaded heavily) represents the section of a tube with walls equal in thickness to the bore. Suppose this to be enlarged by pressure from within until the bore gains the size of the original exterior. Assuming that the metal maintains the same volume, the exterior shaded ring shows the new section; and while the inner circumference is enlarged three times, the outer circumference is enlarged but little more than one and one third times, showing

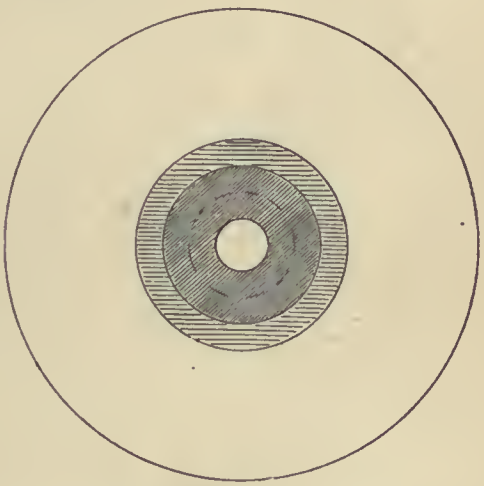


Fig. 4.
EXPANSION OF HOLLOW CYLINDERS.
Illustration of Barlow's Law.

that the strain on the inner metal is much greater than on the outer, as is evident from mere inspection of the figure. The law deduced is that the strain communicated to the metal varies inversely with the square of the distance from the axis. The inmost layer, therefore, might reach its breaking point before the outer metal, and a crack might be started within, which would tear outward so that the tube would rupture before the full strength of the outer metal had been brought into play.

If, however, each layer progressing outward were originally stretched slightly more than the one within, it is evident that the inner layers would be more supported by the outer layers against internal pressure, and the full advantage would be obtained when these strains were so proportioned that the layers would reach their limit of elasticity at the same instant. Then the tube would stand a much higher internal pressure than before. A tube so constructed with walls half a caliber thick (indicated by the dotted circle) would be one and one half times as strong as a ring of homogeneous metal a full caliber thick. Such a construction will evidently bring the inner layers into a state of compression; and this is a decided advantage, as they then can be expanded more before breaking than if in their normal condition.

This law of Barlow, so familiar to scientists, would hardly need to be recalled were it not that it is frequently asked, even now, why we should not make guns of one piece. In practice, the law of variation of strain is more complicated than that given by Barlow, too much so for me to undertake its discussion now. Its application to army gun construction has been ably made by Captain Birnie, of the Ordnance Department of the Army; but the old law, approximately correct, gives a striking illustration of the disadvantage of the solid gun; for, though such initial strains as are described can be produced to some degree in cast iron, yet they cannot be as fully controlled as in the "built-up" guns.

While the ideal of tangential strength is secured more fully by wire wrapping, in which each layer is very thin, difficulties encountered in giving sufficient longitudinal strength by this method of construction have delayed its adoption, and the usual practice is to secure the desired condition, approximately, by the use of successive layers of hoops, made as thin as the conditions of manufacture allow, the great multiplication of very thin hoops being found undesirable.

In these mortars we have almost the only remaining example of the use of cast iron as a part of the system; for, while the modern high-power rifled gun is made up wholly of steel, the inner tube or body of the mortars is of cast iron. The high pressures and strains in the larger guns put the use of cast iron out of the question for them; and even for mortars the all-steel construction presents some decided advantages, since by it a more powerful piece of less weight can be obtained.

In Figure 1 the shape of the cast-iron body is shown, with bore extending through and fitted at the rear with slotted screw threads to hold the screw breech-piece. This body is cylindrical at the rear for over half its length, with a slight shoulder at the front of this portion to prevent slipping backward through the hoops, and a taper thence to the muzzle.

Over this cylindrical portion the figure shows, first, a row of seven hoops. The front hoop has a shoulder at the front, corresponding in use to the shoulder on the body. Outside this row of hoops is another row of six hoops, completing the envelope. One of these outer hoops, the second from the front, carries the trunnions, as shown in the figure. The hoops of each row are shrunk on, so producing the desired strain on the inner metal. Difficulties of construction make it desirable to use several short hoops in place of one long hoop or jacket.

The steel hoops, and other steel parts, are made at the Midvale Steel Works, Philadelphia, and sent, roughly shaped to size, to the contractors for finishing; forty-three mortars being under contract at the South Boston Iron Works, and thirty at the Builders Iron Foundry, Providence. These, with two or three more already built, are all for which Congress has made appropriation, out of the eight hundred needed for our coast.

Beside the steel hoops there are steel parts furnished which go to form the breech mechanism. This is of the interrupted-screw type, commonly known as the French, but really the invention of Chambers, an American.

A full screw is cut on the cylindrical breech block, and then the screw threads are cut away parallel to the axis of the block at three points, leaving three spaces plain and three threaded, equal in width. The recess in the breech has screw threads similarly cut and slotted; so that when the threaded sections of the block are opposite the

slotted sections of the breech, the block can be pushed in. Then one sixth of a turn engages all the threads. The threaded portion, of course, has to be longer than if the full thread were used, but in the latter case the block would have to be turned a great many times to screw it home. When pulled out, the breech block rests on a hinged tray, which swings to one side, uncovering the breech for loading.

The hoops are bored to a definite size, then the cast-iron tube or body is turned to a diameter about five one hundredths of an inch larger than that of the interior of the small hoops. These hoops are expanded by heat until they will slip over the body. They are put on from the rear, singly, and cooled in place; then they are turned off on the exterior with reference to the larger hoops, and the latter are applied in like manner. The front hoop of each row is forced forward to the shoulder, and the rear hoops are forced as close as possible to those in place, in order to make a continuous envelope.

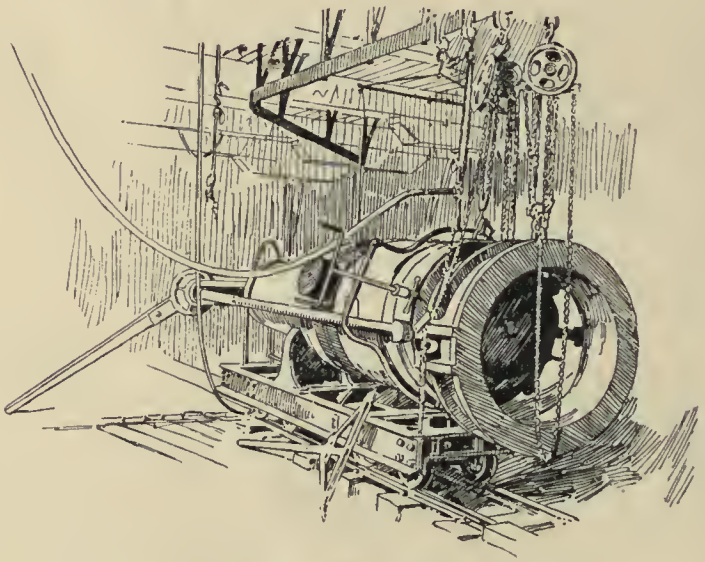


Fig. 5.

Operation of Hooping Mortars.

The application of each row of hoops contracts the bore of the cast-iron body six to eight one thousandths of an inch, although the thickness of the walls is nine and one half inches.

By means of the star gauge the original diameters of the bore are measured at short intervals throughout the length, and remeasure-

ments are made after each row of hoops is applied. The instrument is constructed as follows: Through a brass tube runs a sliding rod, having at the rear end a handle bearing a scale, by means of which, with a vernier on the rear end of the tube, the inspector can read off the movement of the rod. The front end of the rod terminates in a cone, and as it moves forward it presses outward two or three measuring points, arranged radially with reference to the axis, and made to slide through holes in a head attached to the front end of the tube. The instrument is first tried in a gauge-ring of the proper size, and a reading is taken. Then this operation is repeated in the bore, and the difference of reading gives the variation in size from that of the ring.

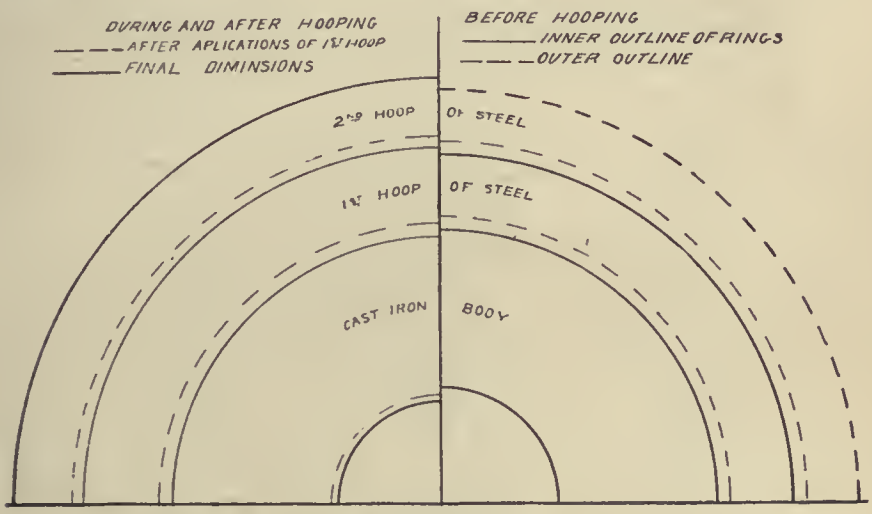


Fig. 6.
Effect of Hooping.

The hoops are heated in a gas furnace to a temperature not greater than 500 to 600° F. above the temperature of the shops, higher temperature being avoided in order to prevent all formation of scale on the interior. This expands the hoop enough to give a play of about five one hundredths of an inch in slipping on the mortar. When the hoop is in place it is clamped securely to the front of the piece by long side bars, drawn up powerfully by hydraulic or screw pressure, and a spray of cold water is at once driven against the outer surface to cool it.

The hoop is cooled at the front first, in order that it may nip there

before it nips at the rear, as otherwise it might shrink away from the front hoop in cooling and leave a wide joint. The clamps are drawn tighter and tighter as the hoop cools, and they are released only when the hoop is firmly shrunk in place. Then another hoop is heated and put on.

Figure 5 shows the apparatus for hooping, the mortar being horizontal during the operation.

Figure 6 illustrates the different steps in the operation of hooping. In the portion to the right the figure shows the sizes of the body and the hoops before they are assembled; in the portion to the left, the effect produced by applying the hoops. By the application of the small hoop to the body, the body is compressed and the hoop enlarged. By the application of the larger hoop the body is still further compressed, the small hoop is reduced in size, but not quite to its original dimensions, and the outer hoop is enlarged.

Figure 7 shows the construction of the mold for casting by the Rodman process of cooling from the interior, a current of water flowing through a hollow core. The mold forms a cylindrical cavity about sixteen feet deep, and the usual hollow core of the Rodman process runs down through the center. A pipe from above runs down within the core nearly to the closed bottom, and supplies a stream of cold water which flows up within the core, outside the pipe, and off by an escape-pipe above. The metal stiffens enough in about twenty-four hours to allow removal of the core. The water is then admitted into the hole itself, though the metal of the casting is still at a red heat. The clay cover of the core is left in when the core is drawn out, and this forms a protective lining to the cavity to prevent excessive chilling of the metal. The figure shows the core extending only part way to the bottom, with the mold following nearly the contour of the cannon; but the present practice is to make the casting cylindrical, and as the mortars are breech-loading, the core extends clear through to the bottom of the mold, where it is stepped into a depression, a pot of cast iron being sunk in the sand at the bottom of the mold, with the edge slightly projecting, so that the molten metal comes in contact with the edge, and forms a joint tight enough to keep the water from escaping after removal of the core. The metal flows down through runners in the sand of the mold, and runs in first at the bottom, side gates from the runners admitting it higher up as the mold fills. These openings

are set oblique to the radius, to give a swirling motion to the metal in the mold, this making it easier to keep the impurities from sticking to the sides.

About twenty tons of pig iron and scraps of former castings are melted and poured in this operation, the reverberatory furnace being used. The cooling takes from seventy to eighty hours. A fire is kept up in the pit outside the mold to check the cooling of the exterior.

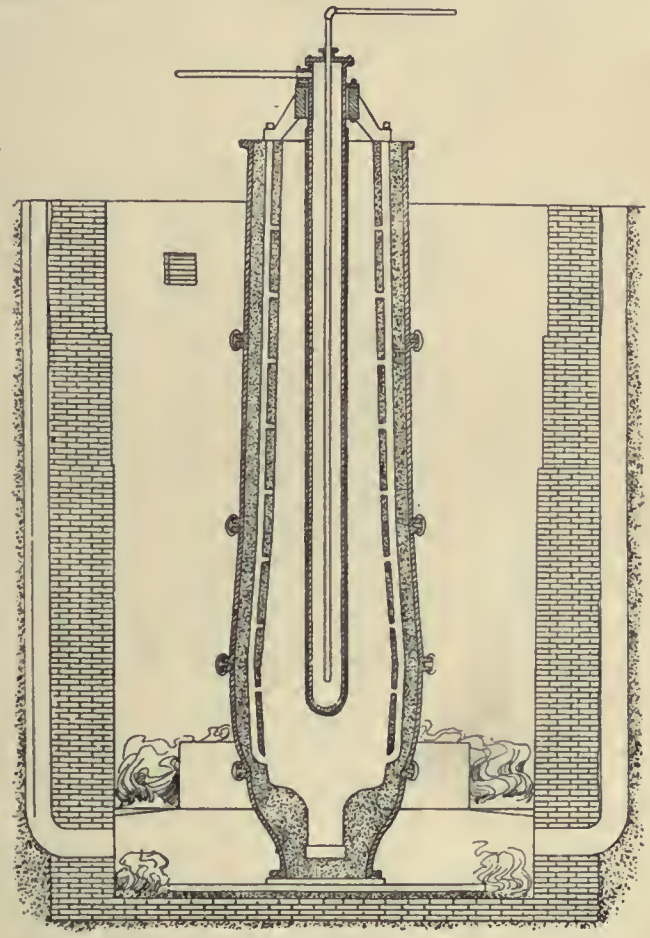


Fig. 7.

Pit and Mold ready for Rodman Casting.

Figure 8 shows the appearance of the casting when removed from the mold. It is a cylinder about thirty-four inches in diameter and fifteen and one half feet long, with a rough bore about ten and one half inches in diameter. The workman is represented as cleaning off the exterior.

The casting is made cylindrical to secure an approximately uniform strain of cooling throughout the casting. A length of about three feet is cut from the top, and five to nine inches from the bottom, or breech end, leaving room between for the body. This middle part is bored out and turned down to the proper size, tapered off at the muzzle, the weight being reduced to about eight tons.

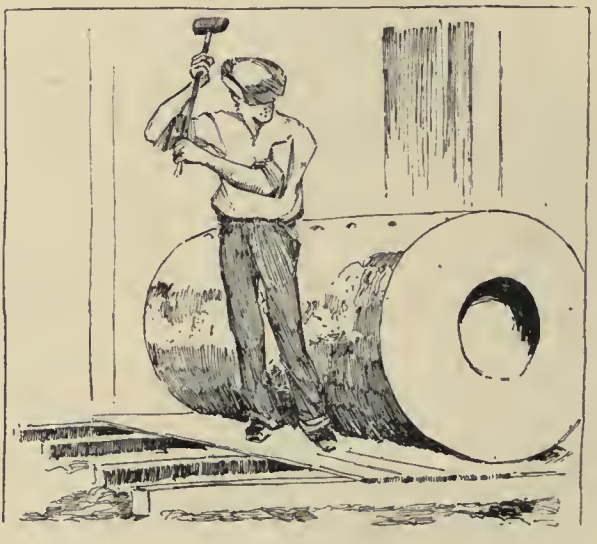


Fig. 8.

Appearance of Casting when taken from the Mold.

The effect of cooling from the interior is to produce a compression of the metal near the core, and an extension of that near the outer surface, this effect depending on the rate of cooling, or the rate of flow of water through the interior. Exactly at what stage of cooling this effect is produced most extensively is not yet determined; but experiments are in progress to determine this, and to find by means of a new thermo-electric pyrometer the time when the molten metal stiffens. It is probable that the greatest effect is produced soon after the

metal stiffens, when the water flows into the hollow itself, the surface exposed to cooling being much greater than when the flow is through the core. The theory is that the metal nearest the core stiffens first, and that the outer layers, forming later, and having to contract more in cooling than the partly cooled metal within, must compress the interior layers. Evidently it will be impossible for the outer layers to shrink the normal amount, and they will be stretched like the outer hoop shown in Figure 6. The metal close to the exterior, however, which may stiffen earlier from radiation of the heat to the outer walls of the mold, seems to show a reversal of these strains. This outer skin is turned off, and measurements of tension are made only on the layers corresponding to the inner and outer ones of the finished body.

Test disks, shown in Figure 9, are cut from the castings close to the top and bottom ends of the body; and the three rings there shown, having a section of one square inch, are removed from the disks, the diameters of these rings being measured before and after removal.

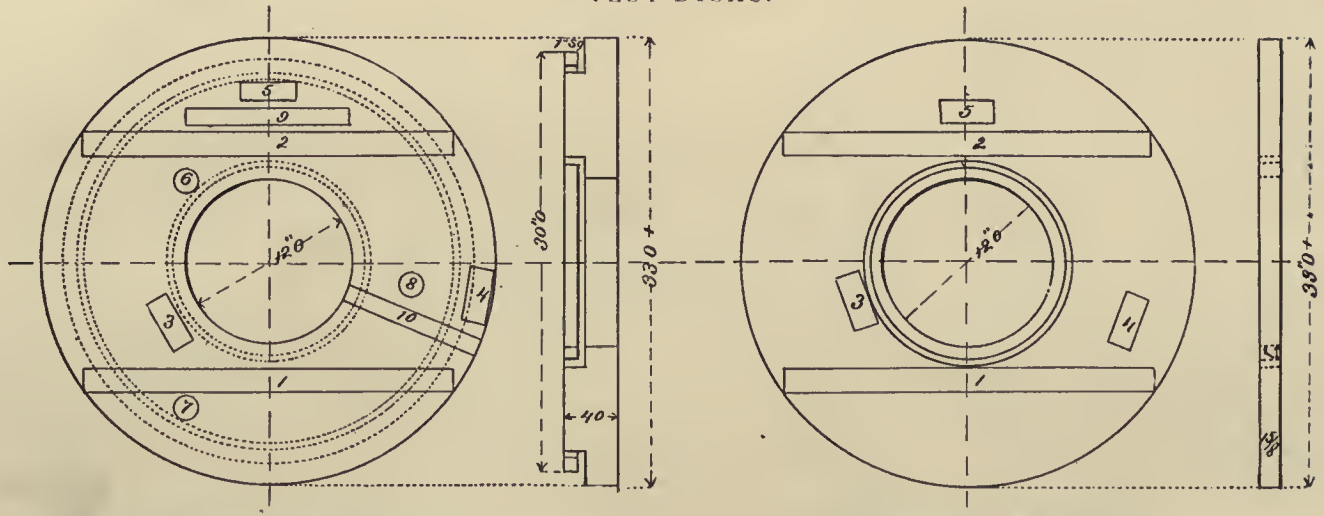
The inner ring at the breech must expand enough to indicate an original compression at the bore of from 5,000 to 9,500 pounds per square inch; and the outer ring must contract, the strain of the exterior iron usually being about 4,000 pounds per square inch. Only a small ring is removed from the muzzle end, and this must indicate a compression lying between 5,000 and 13,000 pounds per square inch, the initial tension being usually greater near the top than near the bottom. The reason for this difference is not clear; but it may be due to greater heat at the top of the pit, outside the mold, than at the bottom, allowing less exterior cooling of the casting at the top.

Figure 10 shows a disk taken from the muzzle end of one casting, and cut into eight successive rings. At the right the curve of strains is shown, the distances below the horizontal line indicating compression, and those above indicating extension, the neutral point being just outside the third ring from the bore. It is to be noted that the outmost ring shows the reduction of strains mentioned above. It appears undesirable, therefore, to leave the outer skin on a finished gun casting, as a line of weakness may be expected a little below the surface.

The old method of determining initial tension was to cut the entire disk through radially at one point, as at the horizontal line, for

Fig. 9.

TEST DISKS.



SPECIMENS FROM BREECH

Nos 1 and 2 Tangential Extension
„ 3 4 & 5 „ Tenacity
„ 6 7 & 8 Longitudinal „
No 9 Tangential Compression
„ 10 Radial „

SPECIMENS FROM MUZZLE

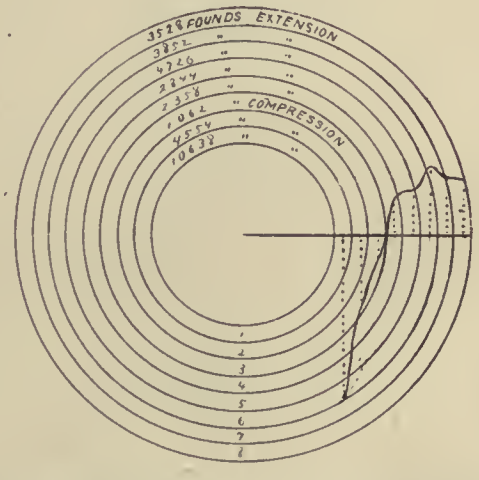
Nos 1 and 2 Tangential Extension
„ 3 4 „ 5 „ Tenacity

instance; and to calculate the tension by the variation in the opening from the width of the original cut. Inspection of the strains indicated in the diagram shows that the variation in the radial slit in the old method gives merely the resultant effect of many varying strains throughout the disk, and no data for calculation of the strain at any particular point of the disk.

The old theory was that the strain of extension on the exterior was equal to that of compression at the bore; but new results show that the former is much less than the latter, and that proper measurement would give about half the compression supposed to exist at the bore in castings tested only by the old method.

Fig. 10.

STRAINS IN RODMAN CASTING.



DISTANCES BELOW HORIZOTAL LINE INDICATE COMPRESSIONS
DISTANCES ABOVE — — .— EXTENSIONS

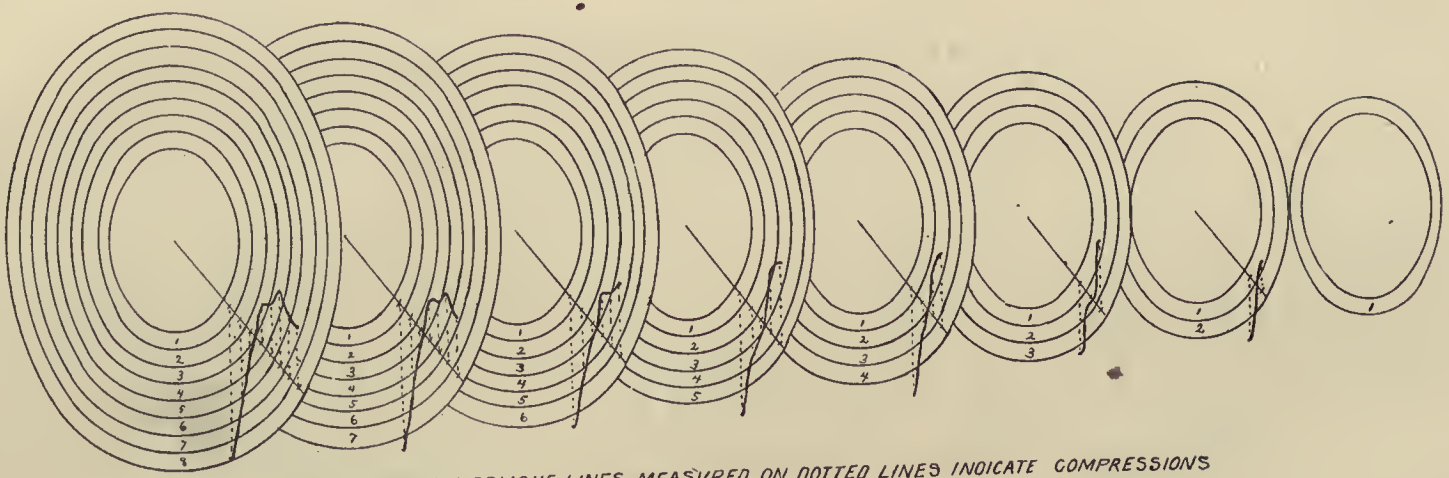
The new method is due to Captain Crozier, of the Ordnance Department of our Army, who, a few years since, at Watertown Arsenal, removed a full disk from a casting and cut it into rings, measuring the expansions and contractions; applying to cast iron methods used by General Mayevski in Russia, and later by Noble in England, for determining strains in steel hoops.

At Providence the process was carried a step further than by Captain Crozier, in that measurements of the rings were made, not

Fig. 11.

RODMAN CASTING.

Change of Strains as Outer Layers are removed.



only before removal of any from the original disk, and after the separation of the rings, but in the disks of different sizes left by cutting off exterior rings. This made it possible to determine the tensions in disks of varying diameters, corresponding to the sizes at different points of the tapering muzzle. The result is given in Figure 11. This shows how the compression of the inner metal decreases as the outer rings are removed, except, perhaps, for the removal of the outmost ring. Also, how the neutral point remains just outside the third ring until the fifth ring is removed, when it shifts to a point between the second and third rings, and the third ring becomes stretched instead of compressed. A reversal again takes place when the third ring is removed, and the second ring becomes stretched in a disk of two rings, the neutral point lying between them.

These recent results of careful and suitable measurements bear out fully the theory of Rodman respecting the advantage of cooling cast-iron guns from the interior, and confirm his genius. Observing the opposite effect in solid cylinders, necessarily cooled from the exterior, where the inner metal tends to become spongy, being drawn outward by clinging to the outer layers, which stiffen first, he introduced the new method to secure sound metal near the bore, and to produce advantageous strains, the old method producing directly opposite strains. The effect of cooling from the exterior is particularly apparent in chilled rolls, which often break apart under the strains produced, and show cavities within.

Even cooling from the interior may be carried on so rapidly that excessive strains will occur, causing rupture at the outer surface; and this has been illustrated in many gun castings, which have burst asunder in the lathe, or even in the mold.

Though tests of specimens cut from the casting near the bore do not necessarily show an increased density, it is probable that the density is increased in the casting itself, the specimen when free recovering its normal size, not having been compressed beyond its elastic limit.

Figure 9 shows the muzzle and breech test disks marked off to show where specimens are to be taken for test: the long ones for elongation and compression; and the short ones for tenacity, density, and hardness. The latter are arranged at different distances from the center. No definite law of variation of tenacity has been found; but the lowest tenacity in any one disk is apt to be found in the middle,

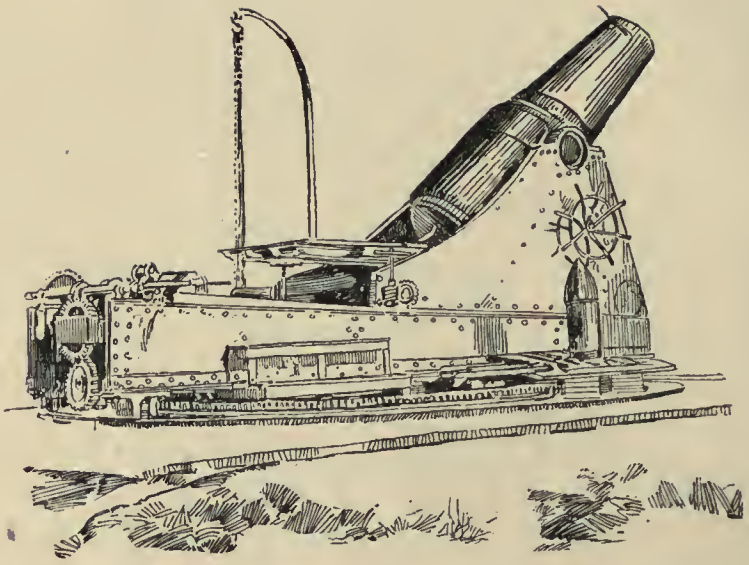


Fig. 12.

B. L. Mortar, old form of Carriage.

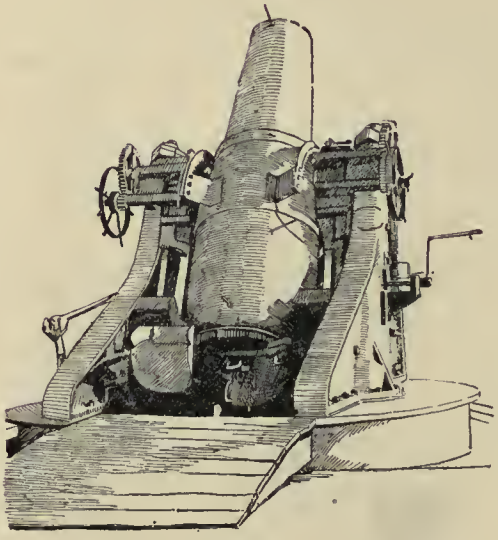


Fig. 13.

The 12-inch B. L. Mortar,

Mounted on the "Easton & Anderson" Carriage.

and the muzzle (near the top of the casting) usually runs higher than the breech. The head of metal seems to give very little advantage. There is the same lack of regularity in density and hardness, but the hardness is usually greatest at the top.

The tenacity must lie between 28,000 and 37,000 pounds per square inch, with an average of at least 30,000 pounds.

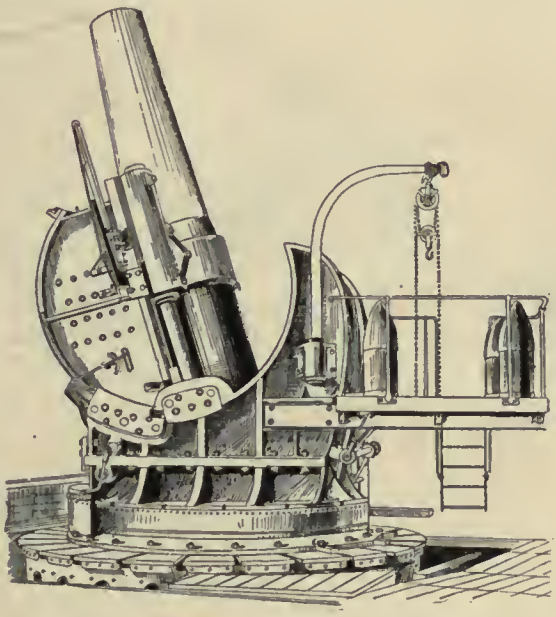


Fig. 14.

WHITWORTH MOUNTING FOR HIGH-ANGLE FIRE.

Figures 12 and 13 show the United States mortar mounted on its carriage; Figure 12, the old form of carriage, without provision for taking up the recoil; Figure 13, the modern carriage, described by Colonel Mansfield, where the recoil downward in an oblique direction is checked by hydraulic or pneumatic buffers. Figure 14 shows the Whitworth carriage with a mortar of English pattern. Figure 15 shows the old-fashioned mortar, mounted for firing.

In this connection it may be said that the old idea of the importance of great weight in cannon to lessen recoil does not apply particularly to mortars, since the methods for checking recoil and

reducing the shock on the carriage have been so fully developed. On the other hand, since the mortar has to be restored to its firing position by means of springs which are not required to bear the full effect of the recoil, advantage in reduced length of springs can be obtained by having a lighter mortar. This would be given by the all-steel construction now proposed, and at the same time a more powerful piece would be obtained, throwing a shell of eight hundred pounds six miles, instead of a shell of six hundred and thirty pounds five miles.

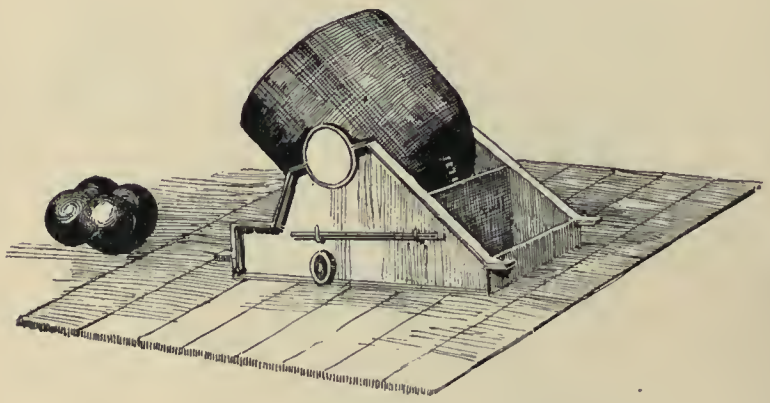


Fig. 15.

13-INCH SEACOAST MORTAR, SMOOTH BORE.

COMMERCIAL ELECTRICITY.

By H. C. Spaulding, S.B.

Read October 22, 1891.

In treating in an informal and brief way of some of the most interesting applications of electrical science which have come under my personal observation, as probably the greater proportion of manufacturing capital and of the time of our ablest inventors are now being devoted to the development of power apparatus rather than to that intended for lighting simply, I propose to give my attention principally to that branch of the science.

It may be well to note at the outset the misapprehension, which is more or less general on the part of the public, as to the scope of electrical power, so called. We very often hear references to the three things — steam power, water power, and electrical power. Of course it is hardly necessary before you to call attention to the fact that "electrical power" should rank rather with shafting and belting, than with the steam engine and the turbine. To be sure, we may get directly from the electric current lighting and heat effects; but for the purposes of our discussion the electric current is simply to be looked upon as a medium of transmission of mechanical energy, and for this very reason can be used economically only where the losses attendant upon the transformation of mechanical to electrical energy and back again will be less than the losses attendant upon the use of steam power or water power, and also where the expense of maintenance and interest on the plant would be less than the expense of fuel, in case the ordinary and more generally known methods were adopted. The limitations, aside from those just mentioned, come in the sizes of the power units delivered, the distance to which the power is to be transmitted, and again in our larger cities, or in communities where the population is compressed, if I may use that term, in the other considerations of convenience and the opinion or prejudice of the public in the place where the power is to be used or distributed.

My subject very naturally divides itself under three general heads of Generation, Transmission, and Utilization, or retransformation of

the electric current. Here again limitations are found, according as we are to use the current entirely for power purposes, entirely for lighting purposes, or partly for one and partly for the other; and again the sub-division as to whether we are using it for lighting large areas, the light being concentrated in different spots, as in the arc system, or for general distribution, as in the incandescent. Passing briefly, then, over the importance of having the generating plant so located and arranged as to get a regular and reliable power, and passing over, also, the construction of the lines, let us take up the third subject, that of the utilization or retransformation of the electric current into mechanical energy. We may start with the simplest form of electric motor, which is illustrated by the standard type manufactured by the Thomson-Houston Company. This motor illustrates very well the simplest form of dynamo-electric machine, either as motor or generator. The magnet is inverted, the ends being simply bored out to receive the armature. It has an ordinary shunt winding, and the whole machine is brought down to the simplest form of the motor and adapted to stationary uses for belting to shafting, and obtaining power in that way. Aside from the ordinary circumstances under which motors can be used exactly as steam engines would be, or other power generators that might be mentioned, such as gas engines, the tendency is to incorporate the motor with the machinery, which is to be run so as to make a complete mechanical unit. Where, a little time ago, we would have had the motor belted or geared to pumps and hoists, we are now more apt to hear them spoken of as electric pumps, electric hoists, etc.

Developing gradually from the primary type of motor to which we supply our electric current, and from which we take the mechanical energy by a belt, we have certain well-defined steps. First, the motor is simply belted to the shafting. Second, the motor is located on a swinging platform and clear of the floor, thus gaining one of the principal advantages of the motor by utilizing space, a feature possible with the electric motor, but not with ordinary engines, simply from the absence of reciprocating motion and "pound." The next step is in the omission of belting, the motor being geared directly to the shafting. This method of distributing power from an engine is rapidly coming to the front. A few large manufacturing concerns in the country to-day have adopted this method. Instead of carrying long lines of shafting for driving belts through their works, they have

placed a large engine at some point convenient to coal and water, running from this a battery of generators, and then distributing from their main lines the current as desired in different sizes. It may be interesting to some to get an idea of what a motor of 15 horse power will carry in ordinary shop practice. Such a motor will carry a 72-inch planer, an 84-inch planer, one 5-foot Warren radial drill, and a large double grinder. This motor was one of a system of about thirty motors running machinery in different parts of a large manufacturing plant from the same generating system, and driven by a large Corliss engine. Another motor of the same size (15 horse power rated, and this would be probably 25 or 30 per cent. under the actual running capacity), geared directly to the shafting so as to do away with belts, carried two 36-inch planers, two 32-inch planers, six 32-inch lathes, one 42-inch lathe, a 48-inch chucking machine, four 5-foot Prentiss radial drills, and a large double grinder. This motor, as I have said, was geared directly to the shaft, and was designed originally for railway work, and known as the single reduction type. In other words, the armature shaft carries a pinion instead of a pulley, which meshes directly with a large gear on the shaft, so that there is very little loss in mechanical transmission at this point.

Naturally the next step in the application of electric motors was in obtaining locomotion. Instead of the motor being stationary and the contact being permanent with the wires supplying the current, this contact was made movable, as we see it in our ordinary trolley street system; and the simplest trucks as used first were made by simply turning one of these stationary motors on its side and placing a pinion on the armature shaft, gearing back twice, owing to the high speed of the armature, the second reduction being with the gear placed on the axle of the car, this axle carrying one end of the magnet to this point, with possibly some springs introduced according to the designer, and the other end being hung in any desirable manner from the frame of the truck. Of course there have been, since that time, hundreds of modifications, some simply in shape and some in winding. The later ones do away with one set of gears, giving what is generally called the single reduction motor. The tendency, naturally, is here also to get the parts as heavy as possible within reasonable limits, and to protect the motor from accidental mechanical injury. In the Robinson radial trucks, which are in use on the West End road, the car is carried on six wheels, and the middle pair of wheels may move laterally in rounding curves.

We now come to the actual application of electric tramway motors to manufacturing processes as adopted in some of the mills, principally in New England, but also in other parts of the United States. The locomotive truck, or locomotive flat car, is, in its electrical features, very much like an ordinary passenger car, though some are much heavier, being designed for shifting as many as five or six loaded freight cars in and about a manufacturing plant, but also being used for carrying loads on its own platform. Others are of a good deal less capacity, in general 3 horse power, and are used for ordinary trucking purposes.

Up to this point these are ordinary applications of simple railway motors to mill tramways for manufacturing purposes. Motors as adapted to transfer tables have not been used to any great extent. I do not believe there are more than three or four in the United States at the present time. There is one at Cheyenne, Wyoming, which would stand as a type of all the rest. Probably one of the earliest transfer tables in the country was at Altoona, Pennsylvania. The Pennsylvania Railroad Company is devoting a good deal of time to the adaptation of electric motors to various purposes. The transfer table at Altoona is very long, having possibly a capacity for three freight cars of the usual size. The current is taken from copper strips which are placed along the under side of timbers parallel with the rails, the brush coming up underneath and rubbing along on these copper strips.

The first drawbridge to be operated by electricity was at Bridgeport, Connecticut; and, as was often the case in the early days of electric power, it required a great deal of demonstration to convince the authorities that it was possible to operate their drawbridge successfully and reliably by this method. People in general, in the early days of the motor business, seemed to think that electric currents were all very well to ring electric bells, and possibly to run sewing machines with; but not really, I think, until the adoption of electric motors on our street railways did the public generally come to appreciate sufficiently the power and possibilities of the force, and to believe that it could be as fully relied upon for power transmission as could line shafting, or a system of ropes or belts. The Rush Street drawbridge, at Chicago, has recently been equipped in a similar manner. At one of the large glass works at Pittsburg a motor has been applied to what might properly be called a transfer table, the glass in the melted state being poured out on a large metallic disk, and then being carried by this

transfer table to the rolls and polishing machines, under which it is treated. This truck is nine feet gauge, and contains some novel features. It displaced the endless rope system which was previously used. The rope had to be renewed about every six months, and was constantly giving trouble.

Of traveling cranes there is one of 20-ton capacity in the factory of the Thomson-Houston Company at Lynn, Massachusetts. Two very similar cranes are in the shops of the Blake Manufacturing Company at Cambridgeport. These cranes are among the few in the country at the present time (I think the first one was put in by the Pennsylvania Steel Company at Steelton) having three separate motors for three separate motions, one large motor being used for hoisting the load; a second for carrying the trolley back and forth on the bridge of the crane; and a third for effecting the motion of the bridge itself up and down the building. The power is supplied in the first place to two copper wires passing over insulated supports on each side of the building above the crane rails, the supports being placed at intervals, and the wire simply lying loose on them. As the bridge passes along, rollers affixed to it, at a sufficient height to clear the guides supporting the insulators, lift the wires, the positive being on one side of the bridge, and the negative on the other. The motor for giving the up and down travel in the building is connected directly with the trolley wire through these rollers. This motor in the 20-ton crane is 3 horse power. The other two motors are placed directly on the trolley. This crane is built by the Morgan Engineering Company, and is their regular type of girder. The trolley carries not only the hoisting motor on it, but also the motor for giving the cross travel on the bridge. The third motor, 3 horse power, is stationary as far as the bridge is concerned, and is placed at one end of it. The controlling levers are placed in a cage hung from one end of the bridge, and there is no mechanical connection between these motors and the drums. On the inside of one of the beams, insulated of course from it, are copper strips. Resting against these copper strips are carbon brushes, which take the current and carry it to the motor, and also down to the operator's cage. Each controlling handle operates a reversing switch and the rheostat for varying the speed of its motor. This crane that I have spoken of was the first crane to have no mechanical connection between the trolley and the controlling cage. The government has now in process of construction some three or four cranes on the same general principle.

Turning now to hoisting arrangements, one of the early types has the motor applied to the hoisting drum through double reducing gear. The pinions in this case, as in the early types of railway motors, were made of rawhide. Some modifications have been tried, such as rawhide and steel in alternate layers. Rawhide, I believe, is being produced by one or two firms by a new process, which is generally considered as meeting the requirements, although that is a point upon which opinions differ. A different type of hoist, with a winch head, is mounted on wheels so as to be portable. Several of these, I think nine or ten, are already in use, and others are being built for the steamship companies, it being a very convenient thing, and very economical as well, for use in unloading vessels. Power is needed at intervals only, so that any method by which it can be quickly obtained when necessary is very desirable. In a case that I have in mind the warehouses come up very near the dock, and along the front of the storehouse are placed service plugs similar to wall pockets for portable lamps, but carrying a current of greater power, and these hoists are moved about on the wharf precisely as donkey engines would be. The hoists are provided with flexible cables, and the plugs at the end fit into these sockets so as to leave as little slack cable as possible. The hoists of this type were designed principally for mining work, the first one being used to handle coal cars in a coal mine in Utah, where the railroad runs along in a valley with high mountains on either side; and a little way up on one of these is situated the coal mine, the vein running almost level, but being a little pitched down toward the slope. The cars are carried out through the tunnel and down the slope to the building in which the coal is picked over and cleaned. The whole plant in this mine is operated by electricity, the power being supplied by two Corliss engines, and used for hoisting the cars, which are run out in trips. They run down to the house, and the empty cars are pulled back at several times the speed that is possible by using mules. The power house is situated on the other side of the valley. Besides the hoist there are various ventilating fans at different points on the mine. The general plan is the same as in the small size, but the brake power is increased considerably, as would be necessary in such a case as this.

In the first part of the work of applying motors to elevator service a good deal of trouble was encountered, owing to the fact that at that time it was supposed to be inadvisable, not to say impossible, to stop

and reverse the armatures of the motors; and the first devices for running the elevators simply comprised the application of the power obtained from the motor to gears and open belt machines, just as gas engines or steam engines would be used, and of course involving the same objections as in any application of power to similar elevator apparatus. As the problem was studied into, and the electrical engineers found that motors could be stopped and reversed with perfect ease, the incorporation of the motor as a part of the mechanism resulted in some machines which are equal in smoothness of motion to hydraulic work. Several machines of this type are running in our large office buildings in Boston. The construction involves the use of worm gearing, the worm being placed over the worm gear on the drum shaft, with suitable brakes to control the mechanism, the rheostat and switch being placed in the car. One little piece of auxiliary apparatus is an automatic brake, which is perfectly simple in its general design. On the drum is placed a strap brake, and this brake is so arranged as to be held tight by a weight sufficient to operate it. Whenever the motor is in use in either direction, or, in other words, whenever the current is turned on, a solenoid coil is energized, the pull on its core overbalancing the effect of the weight on the brake strap. When the current is shut off to stop the elevator, the brake acts automatically as the solenoid is deënergized, and the weight again applies the brake. This device has also been applied to the traveling crane to hold the load whenever the current is shut off. While the direct acting electric elevator machine was being evolved, numerous devices were placed on the market for the application of electric motors to hydraulic elevating plants, some using a switch for starting and stopping the motor gradually as the water was needed, and others having the motor in operation all the time, and pumping the water from the supply tank into the pressure tank; and when the latter was full the valve was automatically opened, which allowed the water to run back from the pressure tank into the supply, so that the water was practically short circuited, and hardly any work was done.

In a type of pump which is perhaps specially adapted to electrical work the shaft carries three plungers placed at angles of 120° to each other, so that the motion is very even. There is no perceptible pull on the armature, and the water thrown out goes out in a very even stream. The results are very good. The pump is manufactured by the Gould Pump Company, at Seneca Falls, New York. The gearing

is double, the plungers single acting and vertical. A modification of this same type has the plungers in horizontal position; another interesting feature being that every section of the pump is like every other section; so that for mine work, if possibly half a dozen pumps are required, each of different capacity, instead of the entire half dozen differing in each part, every section is a duplicate of its corresponding part; so that one complete section kept on hand allows for repairs in case anything breaks. That is, one pump might be a triplex pump, another possibly a duplex, and another possibly a quintuplex pump, with five cylinders, each exactly like its neighbor and exactly like those in the other pumps. One of the largest or most powerful electric pumps yet built was designed for lifting 100 gallons per minute to a height of 1,500 feet. This is for deep mine work in the West. The pump part itself is of the ordinary Knowles mine pump pattern, but spread so as to take in the motor between the center lines of the cylinders. The armature shaft is coupled to what is really an extension of this shaft, carrying two worms, right and left-handed respectively. If only one worm were used to transmit the power to the main gear operating the connecting rods, we should have considerable end pressure on the armature shaft, which for obvious reasons it was desired to avoid. The method adopted for obtaining this result was in using the second worm, meshing with an idler, which also meshed with the large gear wheel just mentioned, thereby balancing the acting forces and doing away entirely with the end thrust on the armature shaft. In order to obtain the best possible lubrication the gears were surrounded by a cast-iron jacket filled with oil, and fitting so closely that the teeth of the gears acted as paddles conveying a continuous supply of oil to the worms. An "alleviator" takes the place of the ordinary air chamber, which, of course, under such tremendous pressure would not do the work, and consists of a number of cylinders of rubber which are compressed by the piston against which the water acts at this point. A twenty-four hours' continuous test of this pump gave results which were exceedingly satisfactory; the efficiency of the entire pump, that is, the ratio between the electric power furnished by the motor and the actual work on the water, being 74 per cent., which is exceedingly good, in view of the fact that worm gearing was used; but I attribute that to the fact that in an ordinary worm gear, unless rolls are used (and certainly I should not want to send roll bearings to that part of the

country), the end of the shaft will have the same surfaces continually in frictional contact, thus preventing thorough lubrication; whereas in this case this end thrust is taken away entirely, and by means of the second worm fresh bearing surfaces are continually presented.

I propose now to pass to some applications of electric machinery to mining processes. The first electric locomotive in the United States that I know of was in the Lykens Valley Mine, and was designed and built under Mr. Schlesinger, then of the Union Electric Manufacturing Company. Mr. Schlesinger is certainly entitled to a great deal of credit for being the first to introduce an electric locomotive in mining operations. The locomotive is now somewhat in the condition of the small boy's jackknife after it had a new handle and a new blade; but following that have come numerous modifications, which have gradually overcome the prejudice of mining engineers against the introduction of electricity in mines for this purpose. On the business side of this question of electric mine locomotion, it is interesting to note the difficulties which are met with in getting a good word from the mining men who have been benefited by the results of the work of the very machinery which it is desired to sell to their competitors. One mining man who adopted this method increased his output about 40 per cent., without increasing his running expenses at all. This he did by changing mule power for electric power for hauling coal. But a man doing that does not want his neighbors to adopt the same means, so that it is very hard to obtain recommendations from the men from whom you most desire them. The second electric locomotive that was built was for the Eric Colliery. One of the next types, and one which is attracting some attention from mining electrical engineers, is known as the "terrapin-back" locomotive. This locomotive weighs complete 21,600 pounds, is for a gauge of three feet, stands about three feet six inches clear from the rails, and handles anywhere from twenty to thirty mine cars at a time. The trolley is of special design, being simply like one section of the ordinary lazy tongs, and allowing for considerable variation in the height of the trolley wire. At the bottom of the two arms are placed gear sectors, so as to keep one side at the same height as the other; and adjustable springs give the tension at this point and keep the arms together, the trolley wheel being carried at the top of the parallelogram. Some improvements, which may not be of special interest to you, are introduced into the controlling mechanism.

A device which has attracted a good deal of attention of late is the reciprocating drill. The two principal experimenters upon it are Mr. Marvin, with the Edison Company, and Mr. Vanderpoele, with the Thomson-Houston Company. The drill consists of two or more solenoid coils, the core carrying directly the drill chuck which holds the drill at the lower end. Now the current is shifted by an arrangement on the commutator, and as it shifts the core is drawn from one end to the other, giving a reciprocating action to the drill. If this meets the expectations of its inventors, it will find a very great market in our deep mining operations in different parts of the country, since the electric current can be carried from the generator at the surface to the working face, that is, to the rock, with much less loss than by the compressed air system. The ordinary compressed air system used in our deep mines has only 30 or 40 per cent., on the average, of the initial power available at the point of operation, while such a device as this should have at least 60 per cent., and very often more under advantageous conditions. Very appropriately, one of the first applications of electric motors to mining operations was in the prospecting drill, used for bringing up a core of the ground to enable one to see whether it would pay to mine or not. In the Diamond prospecting drill the motor furnishes the power for rotating and feeding the drilling cylinder, and also pumps the water to the end of the drill.

Finally, for the measurement of commercial electricity the recording watt meter, so called, of Professor Thomson is now being manufactured for the various currents, 110, 220, and 500. It is exceedingly ingenious, the field coils being connected in series with the line, and the armature, which is placed on a vertical shaft, connected across so that the strength of the fields is governed by the quantity of the current, the strength of the armature by the voltage, and the resulting speed gives a very accurate measure of the quantity of the current passing through in a given time. The slow motion is obtained by the drag of a copper disk, working between the pole pieces of three permanent magnets; and the adjustment of the entire apparatus can be obtained by changing the distance of these magnets from the center of the disk as it revolves at this point. The number of revolutions is registered by ordinary clockwork, and readings are taken as from an ordinary gas meter.

LONG DISTANCE TRANSMISSION OF POWER BY ELECTRICITY.

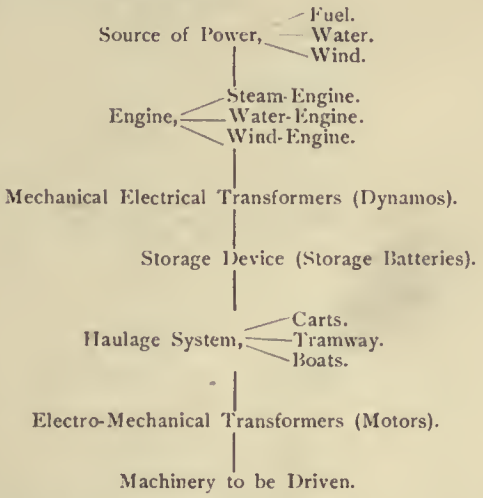
By FRANCIS R. HART.

Read December 29, 1891.

GIVEN a point where power is best obtainable, and another point where such power can be most advantageously used, the problem given to the engineer for solution is that of transmitting this power from the one point to the other. If the transmitting agent is electricity, the method is called one of *electrical power transmission.*

There are two general types of electrical methods that can be employed, (1) haulage, and (2) line transmission.

The first is comparable to the haulage of coal from the mine to a distant factory, there to have its stored energy utilized in mechanical work. The skeleton of such an electrical power transmission system is shown in the following diagram:

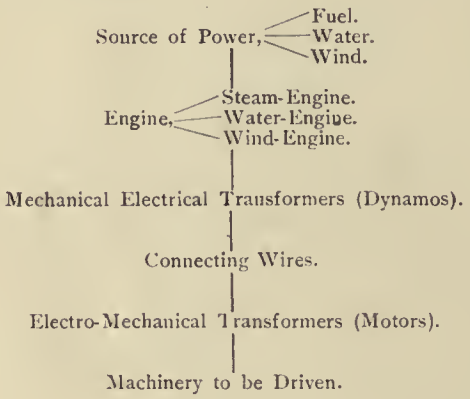


More analogous than the haulage of coal to this method would be a compressed air system. If the steam engine or turbine at the source of power drove air compressors which filled portable vessels

with compressed air, and these vessels were transported and attached to air engines at a distant point, we would have a system directly comparable with the electrical haulage plant.

The storage battery street car is an example of this method of electrical power transmission.

The other general type of electrical methods for distant delivery of power employs a line of conducting wires between dynamo and motor in place of the cumbersome system of haulage. The skeleton of this system is as follows:



The second system has obvious advantages for long distance transmission, and I shall consider with you that only. There are conditions, however, where even for other than tramway work the haulage system may be the only or best system that can be employed.

I have given *fuel*, *water*, and *wind* as the three power sources which may be employed to drive a power system. Some of the uses to which electrical methods of transmitting power may be put are shown in the following table:

Steam:	1. Large number small users obtain more economical power from one engine. 2. Local distribution from lighting companies. 3. Street railways. 4. To replace long shafts. 5. Special works (mines, etc.)
Water:	1. Locate mill with reference to shipping conveniences. 2. Concentration of small water-powers. 3. Carrying power into cities for factories, street railways, etc. 4. Continuous use of power (power during day, and lighting at night).
Wind:	1. Pumping (mines and irrigation); wind engine at best point, and pump anywhere. 2. Lighting with storage cells.

In practice, except for street railway and mining work, fuel is not an economical source of power for long distance transmission plants. Ordinarily it is cheaper to deliver the coal at the distant point and obtain the power directly. In connection, however, with water-power the usefulness of an electrical power system can hardly be exaggerated.

Before describing to you the various systems that can be employed for line electrical power transmission, I wish to call your attention to certain points concerning the efficiency of such a method, and how far we can govern this efficiency. We call the mechanical efficiency of a system the ratio of the power delivered to the dynamo-electric machines at one end of the line, to the power delivered by the electric motors at the distant end.

The commercial efficiency of a dynamo or motor, as I shall show you later, varies with its load. The maximum efficiency of good machines should not be under 90 per cent., and is seldom above 92 per cent. Under the most favorable conditions, then, we must expect a loss of, say 9 per cent. in the dynamo, and 9 per cent. in the motor.

The loss in transmission, due to fall in electrical pressure or "drop" in the line, is governed by the size of the wires, the other conditions remaining the same.

For a long distance transmission plant this will vary from 5 per cent. upwards.

With a loss of 5 per cent. in the line, the total efficiency of transmission will be slightly under 79 per cent.

With a loss of 10 per cent. in the line, the efficiency would be slightly under 75 per cent. We may call 80 per cent. the practical limit of the efficiency with the apparatus of today.

The methods for long distance power transmission by electricity may be divided into three general classes: (1) Those using continuous current; (2) those using alternating current; and (3) regenerating or "motor-dynamo" systems.

I have tabulated the subdivisions of each of these general classes, as follows:

Continuous Current.

- 2-wire
  - Low voltage
    - One machine.
    - Machines in parallel.
  - High voltage
    - One machine.
    - Machines in parallel.
    - Machines in series.
- 3-wire
  - Two machines in series.
  - Machines in multiple series.
- Multiple wire
  - Machines in series.

Alternating Current.

- Alternating single phase
  - Without conversions.
  - With conversions.
- Alternating polyphase
  - Without conversions.
  - With conversions.

Regenerating System.

Alternating-continuous.

Continuous-continuous.

Partial reconversion of any system.

The relative advantages of these systems vary with each particular transmission problem, but in a general way may be tabulated as follows:

	System.	Advantages.	Disadvantages.
Continuous.	2-wire, Low voltage.	Safety, simplicity.	Expense for copper.
	2-wire, High voltage.	Economy, simplicity.	Danger, difficulty of building machines.
	3-wire	Low voltage at machines and saving in copper.	Not saving enough in copper for long distances. Necessity for "balanced" system.
	Multiple-wire . . .	Low voltage at machines and saving in copper.	Not saving enough in copper for long distances. Necessity for "balanced" system.
Alternating.	Single phase . . .	Economy of copper.	Cannot start under load. Low efficiency.
	Polyphase	Economy of copper, synchronous speed unnecessary; applicable to very long distances.	Complexity. Lower efficiency of terminal apparatus. Not as yet "standard" apparatus.
	Motor-dynamo . . .	High voltage transmission. Low voltage delivery.	Expensive. Low efficiency.

The system selected must, first, fill construction and operation conditions; and second, give minimum cost per horse power delivered. There are many factors which govern the selection of a system. For each problem considered there will be found certain fixed and certain unfixed conditions.

In general the fixed factors are as follows:

1. Capacity of source of power.
2. Cost of power at source.
3. Cost of power by other means at point of delivery.
4. Danger considerations at motors.
5. Operation conditions.
6. Construction conditions (length of line, character of country, etc.)

The partly fixed conditions are:

7. Power which must be delivered (*i.e.*, the efficiency of the system).
8. Size and number of delivery units.

The variable conditions are:

9. Initial voltage.
10. Pounds of copper on line.
11. Original cost of all apparatus and construction.
12. Expenses, operating (fixed charges, interest, depreciation, taxes, insurance, etc.)
13. Liability of trouble and stoppages.
14. Danger at station and on line.
15. Convenience in operating, making changes, extensions, etc.

Assuming that the cost of dynamos, motors, etc., will be approximately the same, whatever the initial pressure, I shall show you the great variation in the cost of wire at different pressures. For convenience in working out such problems for rapid and approximate determinations I have drawn a diagram or chart, Figure 1, from which can be determined the pounds of copper required for transmitting any number of horse power from one to twenty miles with an initial pressure of from 1,000 to 10,000 volts, at various losses in transmission. The scale of ordinates on the left gives the total pounds of copper required for one horse power for each mile transmitted, at 5 per cent., 10 per cent., and 20 per cent. loss in transmission. The scale of ordinates on the right gives the area in circular mils of one of the two necessary wires at the same percentage losses.

To illustrate the use of the diagram, suppose we wish to know the weights of copper required for transmitting 100 horse power five miles at 2,000, 3,000, and 10,000 volts, and at 10 per cent. and 20 per cent. losses respectively. By inspection of the diagram we obtain the following figures:

Voltage.	Drop. per cent.	Total Pounds Copper.
2,000	10	$33.6 \times 100 \times 5 = 16\,800$
	20	$16.8 \times 100 \times 5 = 8,400$
3,000	10	$14.8 \times 100 \times 5 = 7,400$
	20	$7.4 \times 100 \times 5 = 3,700$
10,000	10	$1.24 \times 100 \times 5 = 620$
	20	$.62 \times 100 \times 5 = 310$

Very interesting mathematical deductions can be made, showing conditions which give minimum cost, maximum efficiency, etc.; but

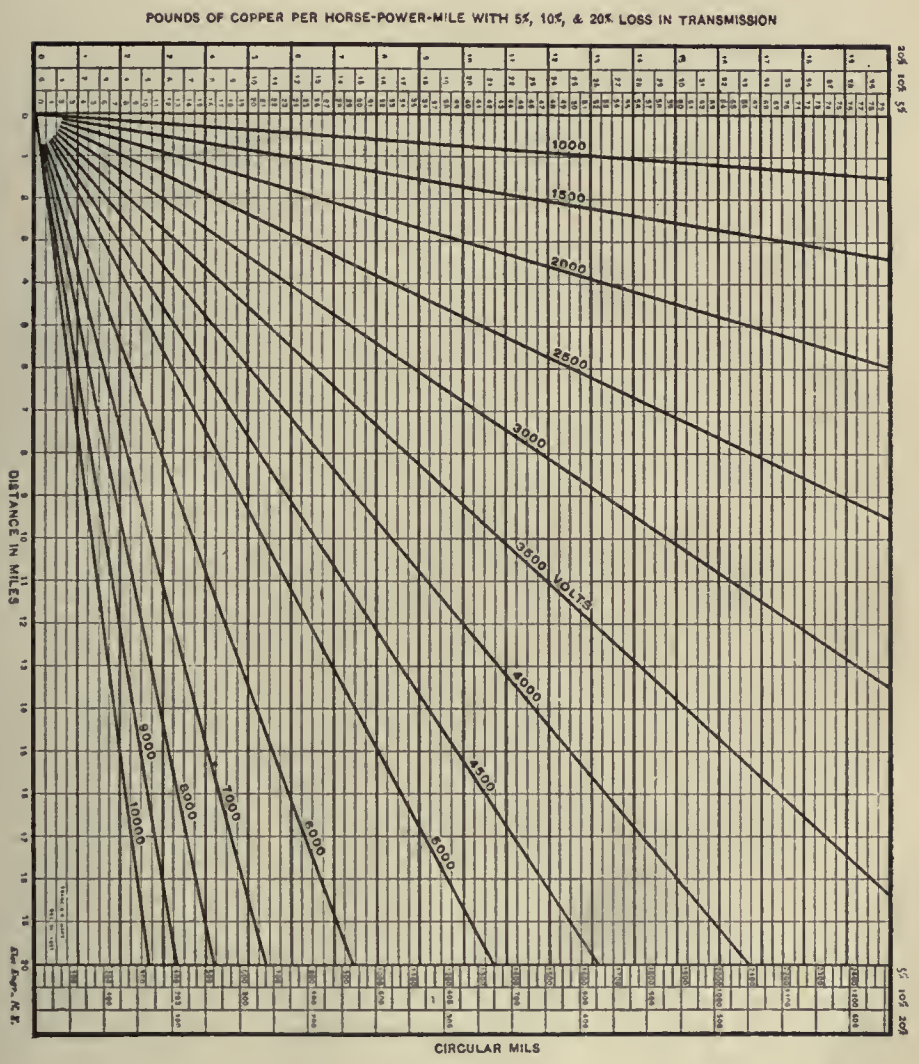


FIG. 1. LONG DISTANCE TRANSMISSION OF POWER BY ELECTRICITY.

in practice these become of small value, as almost invariably there are fixed construction or operation conditions which cannot enter as factors in a derived formula.

In the limited time left to me I can do no more than indicate to you the details of the construction and operation of an electrical power plant. I shall take as an example an installation where the source of power is water. We may separate the details of construction and operation into four heads — hydraulic, mechanical, electrical and financial. Under "hydraulic" come the improvement, control, and care of the water privilege; the selection, placing, and care of turbines, and the turbine regulation.

A good turbine, working at its proper load, will deliver from 75 to 80 per cent. of the total energy of the water. If over or underloaded to any extent, the efficiency is diminished. For the most efficient and economical working of a plant it is, therefore, necessary to exercise care and judgment in the management of the turbines.

Under the head of "mechanical" details of construction and operation belong the selection and care of shafting, gears, belts, clutches, etc. It is customary to so arrange the mechanical parts that any or all turbines or dynamos may be thrown into or out of use without affecting the running of the plant. That is, where possible, all like units should be interchangeable without interfering with the operation of the plant.

Under the head of "electrical" we have: line construction and maintenance; machine construction, installation, and maintenance; construction of switch board; selection, construction, and care of measuring instruments, lightning arresters, etc.; provision for extra parts of machinery, and conveniences for rapidly making necessary changes.

The same care exercised in keeping the turbines at maximum efficiency should be applied to the dynamos. The curve, Figure 2, shows the average variation in efficiency with load of dynamo-electric machines of from 25 horse power to 75 horse power.

In all such plants complete records, in the form of daily reports, should be kept of pressures, currents, speeds, heights of water, time machines are started and stopped, etc. A monthly statement should be made, showing the averages of the daily reports and all expenses for the month. It is convenient to deduce from each monthly statement, for a uniform basis of comparison, the cost per horse power delivered for a stated time.

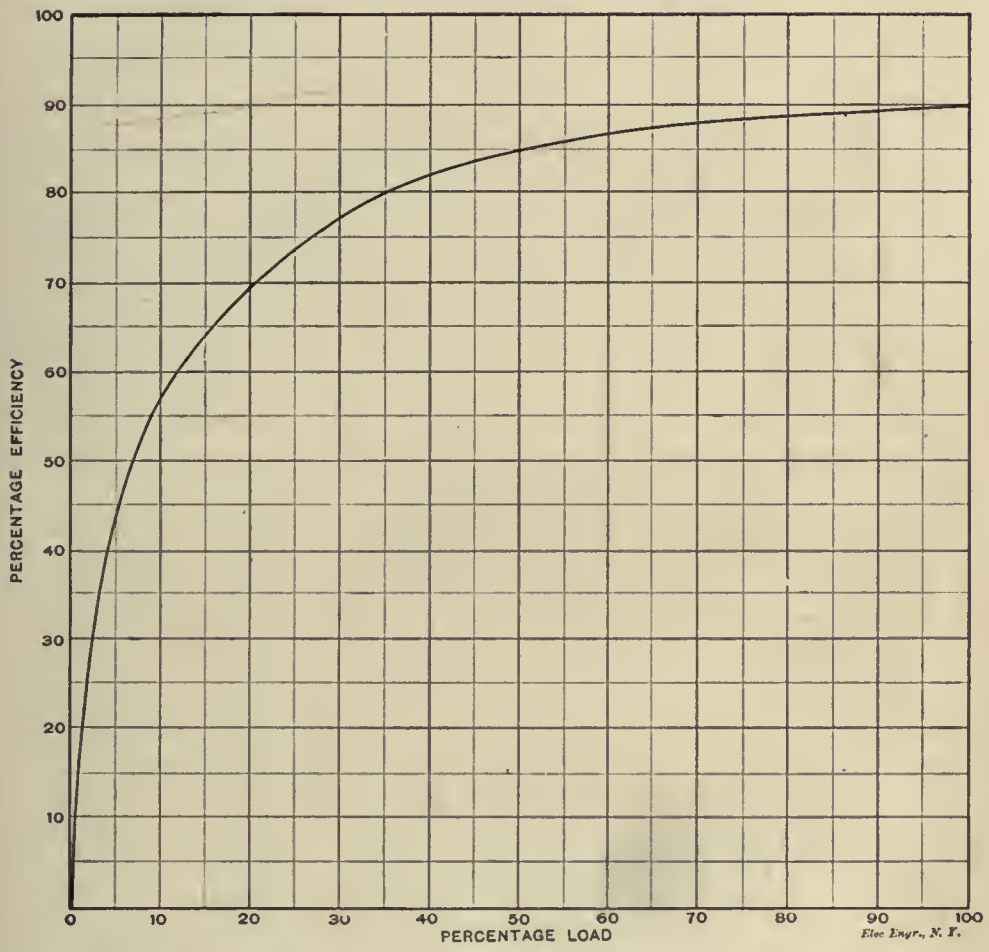


FIG. 2. LONG DISTANCE TRANSMISSION OF POWER BY ELECTRICITY.

ON SOME RECENT DISCOVERIES AND PROBLEMS IN THE ANATOMY AND PHYSIOLOGY OF THE BRAIN.

By JAMES J. PUTNAM, M.D.

Abstract of a paper read February 11, 1892.

Previous to 1870 it was thought that the whole brain acted as a unit, the physiologists of the day believing that they had ascertained through convincing experiments that when any portion of the hemispheres, at any rate, was injured, the functions of the brain suffered a general reduction, and that localized symptoms were not produced.

If one reads the books of the date of Longet, however, indications are noticeable that this opinion was felt to need modification; and in 1861 Broca published his remarkable discovery, indicating, on the basis of post-mortem examinations, that destruction of the third frontal convolution gave rise to aphasia.

In 1870 two remarkable series of investigations were published, which initiated a new epoch in our knowledge of the anatomy and physiology of the brain. The observations of Fritsch and Hitzig in Germany were the first to see the light; but the experiments of Ferrier, which appeared soon after, had been conducted without a knowledge of what the German investigators were doing. These two series of experiments were both of them set in motion by theoretical considerations; but I shall refer to only one of these, that of Dr. Hughlings Jackson, of London, who has always been the philosopher among physicians. He said to himself that there must be parts of the brain in which the "raw materials of consciousness," the ideas of color, form, sound, motion, and the like, must lie separate from one another. He further noted the fact that when epileptic attacks occurred it was common for the spasm to begin with contractions of a few muscles — those of the hand or face preëminently — and then to spread to the rest of the body in regular order, and offered the hypothesis that, corresponding to the order of succession in which the different parts became involved, there would be found a mosaic of centers in

the cortex of the brain. . . . Dr. Ferrier's investigations were begun in order to test this hypothesis.

It would be impossible for me to pass in detail over the wonderful series of experiments which have been made since that day, and I can only indicate their general character and general results.

In character they have consisted in stimulation of the surface and deeper lying portions of the brain by electricity and other means, and in the removal by excision, cauterization, a stream of water, etc., of different cortical areas. [Dr. Putnam pointed out on the models and diagrams taken from Ferrier, Horseley, and others, the position of the centers corresponding to the movements of the face, hand, arm, leg, trunk, etc., the area concerned in motor aphasia, the seat of vision, of hearing, etc.] If the fissure of Rolando is taken as a line of departure, running as it does across the convex surface of the brain, the motor areas may be found to lie immediately adjoining this fissure, while the occipital lobe is concerned mainly with vision, and the temporo-spheroidal lobe with hearing. The seat of the sensory functions of the skin occupies, probably, more than one place, since sensory impairment follows injury in the so-called motor area (which, by the way, is really an area for the sensation of motion performed), and also, in all probability, lesions of the so-called limbic lobe on the median surface of the brain. The so-called centers do not form a mosaic, but overlap, and the different layers of the cortex perhaps have different functions, sensory, motor, and the like; the cortex contains, also, centers for thermo-taxis and respiration, and, in short, for all the functions of the body. The anterior lobes of the brain do not seem to be much concerned in motion or sensation, and even when they are badly damaged, as happened in the celebrated "Crowbar Case," the patient may retain the ordinary functions of life. At the same time it is probable that, since this part of the brain increases in size as we ascend the animal scale, some of the most important mechanisms for the higher intellectual life reside in it.

[The next subject taken up was the practical clinical results of these discoveries, and the general character of the surgical operations for tumors, abscesses, and the like, was described. The theory that epilepsy may be due to a localized cerebral lesion, and therefore susceptible to cure by cortical excision, was discussed, but on the whole this theory was discarded, except for cases of definitely localized irritation.]

As to the question how the brain acts, we find ourselves forced to readopt, in some measure, the views of the older physiologists, though using them in a new sense. In other words, the essence of brain activity is in the arrangements that exist for the association of the functions of the different parts on an enormous scale, and the bulk of the brain is made up of associating tracts of nerve fibers, which lie packed together so closely that on cross section the surface appears as homogeneous as a piece of cheese. If we go down to the lowest vertebrate known, the *amphioxus*, or lancetfish, we find a brain that is scarcely larger than the spinal cord, of which it forms the extremity. Such an animal as this gets on about as well without his brain as with it, the spinal cord being sufficient for life and most of its functions. As we ascend in the scale, we find the functions of the brain becoming steadily more important, and the spinal cord taking a less and less independent place. Even the dog, however, will bear the loss of the hemispheres of the brain fairly well, if sufficient time is allowed him to gradually accustom himself to do without them [Goltz]. In the lower vertebrates the functions of vision and sight are rudimentary, and serve only to guide instinctive acts which result quickly in motion. In the higher animals these functions occupy a relatively enormous area, and are concerned in every thought of the mind, either directly or through symbols. We can perhaps study out the general mode of action of the brain the best by taking one example, the function of language.

When the child learns to speak, his first effort is usually to reproduce a sound which he hears, connecting it very early, though not necessarily at first, with the sight of an object or some more or less complicated idea. His next step is to apply his sound or word to a class of related objects or ideas, the word "bread," for example, being often used for different kinds of food; and then to discover that this expression has been outgrown, and that a differentiation is needed.

However far we trace this process we find the same factors at work, and however complicated language becomes, we test its degree of perfection by the richness of its individual symbols and the differentiation in meaning of which its symbols admit. I beg you now to note that the richness of meaning of a given symbol implies a corresponding coördination or focusing of a correspondingly great number of cerebral functions. Think, for example, what such a word

as "home" or "patriotism" is to the cultivated or fine person, compared to the rude or savage person; or take such a word as "justice," and consider what it means to the primitive man, who knows no one beyond his family or tribe toward whom he is called upon to show consideration, and what it means to the true philanthropist, who regards himself as akin to all the world. To this increased wealth of meaning of the word an increased coördination of cerebral processes corresponds. The necessity for the coördinated action of the various parts of the brain in speech is shown not only by the study of word symbols, but by studying the derangements of speech known as aphasia. [Dr. Putnam then pointed out on the model the way in which lesions of the motor speech area on the one hand, and the sensory and visual area on the other, and the tracts that unite them, may interfere with the speech function.] When we speak a sentence, that sentence rings in the inner ear (auditory cortical area) before it is uttered, and without this it cannot be spoken correctly. The different forms of aphasia occur to a slight degree among healthy persons, under the influence of fatigue, embarrassment, and the like. The fact that we are not conscious of all this intricate brain action which precedes some overt act or utterance does not show that it is not there. Consciousness has been described by one writer as like the little ripples that are seen on the surface of the sea, which strongly attract our attention, while we hardly recognize the existence of the great waves moving underneath.

Two general facts of recent discovery are of interest. One is that, in spite of these intricate arrangements for associated activity of the different parts of the brain, no two ganglion cells are actually united. The fibers grow out from each cell, forming long processes which terminate in the neighborhood of other cells, but do not actually enter into connection with them. So it would seem that the associated action of the different ganglion cells must take place by a process something like induction.

Another point of interest is that it has recently been discovered by Mr. Hodge, of Clark University, that when ganglion cells enter into activity demonstrable changes take place in them, leading to a rarefaction of the protoplasm and a condensation of the nucleus.

Finally, the conclusion may be insisted on strongly that a study of the evolution and functions of the brain leads us irresistibly to the conclusion that it is intended that in the "redistribution of

force for useful ends" all the activity of the brain should result in definite acts, just as much as the nervous arrangements which govern the action of the heart or the other viscera, and the brain should eventually give back all the force that it receives, as the cushion of the billiard table throws back the ball.

If a great deal of our cerebral activity seems to be a contrivance for luxury or idle pleasure, this is because we are not living up to our possibilities. The best educational developments of the present day are those that teach efficiency combined with the power of associating and focusing for a given end the largest possible number of associations and ideas.

A large class of nervous disorders to which modern society is prone are those which come with an undue development of the functions of perception and sensation over those of effective action. A man must not be a narrow-minded Philistine, on the one hand, but on the other, he must not be a sentimentalist. When efficiency is secured at the expense of broadmindedness, we have the narrow man, whose thought symbols are meager in their content. On the other hand, effective action is, after all, a higher function than simple appreciation. Thus the function of speech is a higher function than that of simply understanding, provided that the subject matter in both cases is the same; and so in the neurasthenic and hysteric brain we find a rank overgrowth of sensibilities which cannot be utilized in action, and to counteract the tendency toward this condition is one of the highest functions of education.

THE HOLLERITH ELECTRIC CENSUS SYSTEM.

By T. C. MARTIN, Editor of the "Electrical Engineer," New York.

Abstract of a paper read March 10, 1892.

It was long since evident that, with our rapidly growing population and the universal desire to see the returns as soon as possible, the old methods of compiling a census would not do. The appointment of the Superintendent of the Census was soon followed by the naming of a commission to advise him as to the methods to adopt of tabulating census data. To this commission three schemes of tabulation were presented. That submitted by Mr. W. C. Hunt proposed to transfer the details given on the census enumerators' schedules to cards, distinctions being made in part by the color of the ink, and in part by writing on them, the results being reached afterwards by hand sorting and counting. The next plan, that of Mr. C. F. Pidgin, contemplated the use of "chips," which should be duly sorted and counted. These "chips" were to be slips of paper of various colors, embracing data printed in different colors, so as to indicate the readings of the schedules. Last came the system of Dr. Herman Hollerith. In this plan the information given in the schedules was to be expressed upon cards by punching holes in them in certain positions. The tabulation from these cards was to be made by passing them through a press which would register their indications electrically on dials.

These three methods were put to the test, four enumeration districts of the census of 1880, in the city of St. Louis, being taken. It was found that the time occupied in transcribing their contents by the Hollerith method was 72 hours, 27 minutes; by the Hunt method, 144 hours, 25 minutes; by the Pidgin method, 110 hours, 56 minutes. The time occupied in tabulating was found to be as follows: By Hollerith's electrical counters, 5 hours, 28 minutes; by the Hunt slips, 55 hours, 22 minutes; by the Pidgin "chips," 44 hours, 41 minutes. The Commission estimated that on a basis of 65,000,000 pop-

ulation the saving with the Hollerith apparatus would nearly reach $600,000. As a matter of fact the saving is 40 per cent. more than was expected. It is needless to add that Mr. Hollerith's system was adopted.

Each of the broadsheet schedules of the eleventh census provided for some thirty details regarding any of ten individuals. Such a sheet would therefore serve for a family or household of ten people; but as a matter of fact it was found that the average fell slightly below five, so that in the neighborhood of 13,000,000 of these schedules were sent into the Census Office. The first thing to be done with these records was to get at their gross totals, and upon this work the Hollerith system was first put. The machine was fitted with a small numbered keyboard, the keys being connected electrically with the dials, which were so arranged that one of them would furnish the grand total of families as a check against the separate totals of the others for families of different sizes. The keys were in either two or three rows, of which the top one, stamped from 1 to 8, recorded the number of families in each house. The other two rows, nearer the manipulator, were numbered from 1 to 10 and 11 to 20, and recorded the persons in each family. The enumerators having given this information at the top of their schedules it was easy to transfer the items to the dials of the machine, or make special note of such rare families as exceeded twenty members. Evidently the multiplying of the number of families by the number of persons in each class would, if correct, yield the result indicated by the "total;" and hence the work was capable of very thorough check.

In this manner the first or "dwelling house" count of the whole population of the United States as it stood on June 1, 1890, was made. Practically only six weeks were needed for the task. The last returns were not received until November 10, but the official count of "62,622,250" was issued on December 12. Yet all the figures had been thoroughly checked; in reality, the people of the United States had been counted twice.

The next step was to transfer the particulars as to each individual of the 62,000,000 from these 13,000,000 schedules, and for this purpose Mr. Hollerith had devised the machine known as the "keyboard punch." It is about the size of a typewriter tray, having in front a perforated punch board of celluloid. Over this keyboard swings freely a sharp index finger, whose movement, after the manner of a panto-

graph, is repeated at the rear by a punch. The movement of the punch is limited between two guides, upon which are placed thin manilla cards $6\frac{5}{8}$ inches long by $3\frac{1}{4}$ high. The keyboard has twelve rows of twenty holes, and each hole has its distinctive lettering or number that corresponds to the inquiry and answer respecting each person. Hence, when the index finger is pressed down into any one of these holes the punch at the back responds by stamping out a hole in the manilla card. At first glance, perhaps, the keyboard looks complicated, but it is scientifically grouped and is very readily learned. For such inquiries as are answered by one of a very few possible classes — sex, for example — the answer is simply "male" or "female," or "M" and "F." Where, however, the answers would cover a wider range of classification, as in age, running from one to one hundred, recourse is had to a combination of two holes; the first indicating a group, as from twenty-five to twenty-nine years, while the second hole designates the detail single year in that group. That the work of punching became as easy as any other task requiring ordinary intelligence is shown in the fact that the estimated average of 500 cards per day per clerk resolved itself very soon into an actual average of 700. It is stated that some of the more expert punchers, working from 9 A.M. to 4 P.M., have done 1,100 cards, with an aggregate of 18,700 holes.

After the cards left the punching clerks they were kept in their enumeration districts, and they had to be further punched to show the exact locality they belonged to, *i.e.*, the civil division of which the enumeration district formed a part. For this purpose the space of about one inch across the left-hand end of the card was left blank, no portion to the left of a fictitious line being punched on the keyboard punch. This space is further divided by imaginary lines into forty-eight squares, in the combinations of which every enumeration district can be recorded, and it is perforated by means of the "gang punch." The combination for any given enumeration district is arranged in this, and then all the cards of that district are passed through. From three to six cards can be punched at a time (hence the name), and pressure can be applied by either the hand or the foot. When this is done the cards are complete.

The percentage of bad work was very small. Where the errors were "inconsistent," *e.g.*, where a child of ten would be registered as of no sex, the machine would sooner or later spot the mistake and

refuse to count the card. As to "consistent" errors, those, namely, in which a district messenger might be punched as a professional runner, it is safe to say that such errors were necessarily very few, if only for the reason that it is at least as easy to punch in the right hole as in the wrong one; and these errors would, moreover, on a mathematical basis, be less likely to occur in dealing with holes that are a quarter of an inch apart than where the same record was made under the old system on lines only one eighth of an inch apart on large tally sheets. But besides the natural checks on errors consistent and inconsistent, there was the further check of a frequent examination of the work, as it proceeded day by day and hour by hour.

The next step was to group and count the 1,000,000,000 facts contained by these cards, and this was done by the Hollerith machines. These consist of three main parts; namely, the press or circuit-closing device, the dials or counters, and the sorting boxes. The press consists of a hard rubber plate, provided with three hundred and sixteen holes or pockets, the relative positions of which correspond with those of the holes in the keyboard and "gang punches." Each of these pockets is partially filled with mercury, and they are thus in electrical connection when the circuit is closed. Above the hard rubber plate swings a reciprocating pin box, which is provided with a number of projecting spring-actuated points, so hung as to drop exactly into the center of the little mercury cups below. These pins are so connected that when a punched card is laid on the rubber plate against the guides or stops, and the box is brought down, all the pins that are stopped by the unpunched surface will be pressed back, while those that correspond with punched spaces pass through, close the circuit and count on the dials.

The front of each counter is three inches square. Each dial face is divided into one hundred parts, and two hands travel over the face, one counting units and the other hundreds. The train of clockwork is operated electrically by means of the electro-magnet, whose armature as it moves each time the circuit is closed carries the unit hand forward one division, while every complete revolution actuates a carrying device which causes the hundred hand to count. In this way each dial will register up to 10,000. A noteworthy feature of these ingenious little dials is that they can quickly be reset at zero, while they are also removable and interchangeable. The electrical connections are made simply by slipping them into the frames and clips in the upper part of the machine.

The third element in the system is the sorting box. The box is divided into numerous compartments, each of which is kept closed by a lid. The lid is held closed against the tension of the spring by a catch at the free end of the armature. If the circuit is closed by the press on the machine, through the electro-magnet, the armature is pulled down, releasing the trigger of the lid, which is at once thrown up by the spring, and remains open until put down by a slight touch of the operator's hand.

If, now, it is desired to know in a given enumeration district, or all of them, the number of males or females, white and colored, single, married, widowed, etc., the binding posts of the switch board corresponding with these data are connected with the binding posts of the dials on which these items are counted. If it is also desired to assort the cards according to age groups, for example, the binding posts of the switch board representing such groups are connected with the clips into which the sorting-box plug fits. The circuits being thus prepared, when a card is placed in position in the press, and the handle of the pin box is depressed by the operator so that the circuit is closed through the holes in the card, not only will the registration be effected on the counting dials, but the single sorting box that has been selected for a given age group is opened. The operator releases the handle of the pin box, removes the card deftly from the press, deposits it in the open compartment with the right hand, and pats the lid down again, at the same time bringing another card into position under the press with the left hand. It is done much more quickly than it is described. When all the cards in the tin case of any district have thus gone through the press, the record taken from the dials will show the number of males, females, white, colored, etc., while the cards will have been assorted into age groups.

But the Hollerith machine is capable of much more than this. In statistical work it is found that the most valuable information does not consist in these elementary items, but in facts that are more difficult to obtain, namely, combinations of these items. Thus it is interesting to know how many dwellers in this country are males, and how many females; also, how many are white and how many are colored. But it is at least as essential to know how many of the white males are native born or foreign born, and how many are the children of native born or foreign parents. Hence it is desirable to find means for counting, not simply the number of white males, but the number of

white males, native born, of native parents. Mr. Hollerith's machines do this as easily as they do the lighter work. The old familiar principle of the relay is brought into play very ingeniously by means of a special but simple form. These relays are mounted together in the racks at the bottom of the machine. It is simply a question of arranging the counting dials and the relays, or, if desired, the sorting boxes can be treated in the same way. When the machine is once connected up, the combination sought yields its results just as readily as though it were a single item. It is obvious, too, that multiple contact relays could be used, carrying the ability of the machine even further, if one wanted to do it in that fashion. Moreover, the machine is not confined to tallying one set of facts, but will take several at one and the same time, being limited only by the number of counting dials. The machine also lends itself to analytical work, not less than synthetical. In statistical investigation the analysis naturally becomes finer as the area enlarges, and here the sorting box is of great service. As has already been stated, the cards are primarily massed in enumerating districts. For such small areas the information required groups the population under comparatively few heads. In practice it is found that such classification can generally be counted on the forty dials that the machine embraces normally as a full equipment, and the arrangement is made accordingly. But while counting this classification the cards can also be assorted into groups that will form the basis of the analysis for the next larger group of territorial areas; so that if the cards are divided into twenty groups we shall have at the next handling of the cards a classification of 20×40, or 800 statistical heads. If at the next step we subdivide each one of these twenty groups into twenty more, the third handling of the cards will give us $20 \times 20 \times 40$, or no fewer than 16,000 statistical heads. Thus a few fine manipulations will give an extraordinarily fine degree of analysis, and the compilation will have a value from its minuteness that could be reached in no other way.

It is estimated that each of the machines thus compiles and registers information daily that would require the efforts of twenty clerks, if sought by the old system of tally sheets. But that is not all. Added to the ability to secure special details, finer analysis and the economy in time and labor, we have the greater accuracy. The machine automatically throws out any card that is wrong. Suppose,

for instance, that age or sex has not been punched. Where there should be a hole for the plunger pin to go through, closing the circuit, the card is intact. The circuit stays open, and the monitor bell just to the left of the press refuses to give its signal of correctness. It is then a very easy matter to refer back to the enumerator's schedule, and fill up the deficiency by the paradoxical process of making a hole. Suppose it be desired to connect up the machine so that only cards for New York should be counted. A missorted card belonging to Chicago would at once be rejected. The "gang punches" of the two cities not agreeing, the wrong cards would leave the circuit open. Moreover, the same indications are made, of course, by the same holes for all like data, so that one could run a wire straight through a mile of them. But this could evidently not be done with written cards, and the digging through the 150 tons of written cards for such as had gone astray would be a job preferably postponed until census clerks were clairvoyants.

It is evident that the probabilities of error in this wonderful counting apparatus in reality narrow themselves down to the punching; and, as has been pointed out, even then the only errors that escape detection are those in which the information given, while it may not furnish the exact fact, is still consistent with the other facts punched. Even these could be eliminated by comparison or check of every card, and it is surely hypercritical to complain of results so close to absolute accuracy. It is to be borne in mind, too, that a card wrongly punched involves only the possible miscounting of a single unit, whereas in all previous methods the counting up on sheets has involved miscount at each footing up of a column.

A HAND-TELESCOPE FOR STADIA-WORK.

By ROBERT H. RICHARDS, S.B.

Read March 24, 1892.

If one holds up a prism or wedge of glass with narrow angle, say 1° to 2°, and compares the transmitted image with the image seen above or below the prism, the former will be found to be thrown to one side by an amount varying with the angle of the wedge. Speaking of the two rays as the direct ray and the bent ray, we may say that when the bisecting plane of the prism is at right angles to the line of sight, the angle between the direct ray and the bent ray will be constant for any given prism.

If now we place a prism or wedge of glass in such a position that it half covers the objective of a telescope, we shall obtain, on looking through it, two images of every object seen — one image by the direct ray, which comes through the uncovered half of the objective; the other which comes through the prism, and is the image by the bent ray. The angle of divergence of these two rays will be constant and unalterable, whether the telescope is directed to a near object, with its eyepiece at increased distance from its objective, or upon a distant object, with eyepiece nearer to the objective. That is to say, if the "throw" or apparent dislocation of the image is one foot in one hundred feet, it will be two feet in two hundred, ten feet in a thousand, and so on.

The usual form of stadia telescope has at the focal point of the objective two spider lines, placed at a definite distance apart, and intended, let us say, to represent a throw of one foot in one hundred feet. But as the distance between lines remains the same, while the distance from the objective to the webs differs with every variation in the distance of observed objects, it follows that the angle between the lines of sight which these two spider lines define cannot be constant, but must vary with every increase or decrease of distance between the instrument and the object viewed.

If in Figure 1 we represent three positions of the spider lines by

W_1, W_2, W_3, and three objects corresponding to those positions by O_1, O_2, O_3, at near, medium, and distant positions, we see at a glance, from the figure, that there is no constant angle represented by the spider lines, and that the only way to graduate a rod for the practice of stadia measurement by this method is to determine values for one foot at a sufficient number of distances, and to provide the rod with a graded scale accordingly.

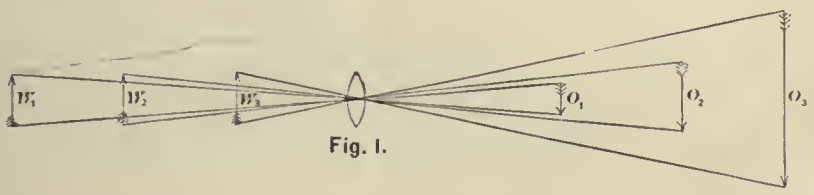


Fig. 1.

The prismatic stadia telescope, on the other hand, has a constant angle for all distances and all focal lengths, and when the factor has once been obtained it may be used to graduate a rod with uniform scale from end to end.

Again, the usual stadia instrument involves two points of observation. The operator adjusts the lower spider line on the zero of the rod, and then observes the reading of the upper line on the rod. There are, therefore, two personal equations in the operation of taking readings. With the prismatic stadia telescope, on the other hand, both observations are made at once, just as the sailor, in taking the altitude of the sun at sea, brings the sun's image to the horizon and observes the contact. Only one personal equation is thus involved.

The spider-line stadia telescope cannot be used by simply holding it in the hand, but requires a firm support; for if it were used in the hand the first line would wander from the zero on the rod before the reading of the second line had been taken. The prismatic stadia telescope, on the contrary, can be used in the hand, just as a sextant is. Nevertheless, as the readings to be made with it are much finer than those of the sextant, a support will be preferred for most purposes.

The one great advantage of the spider line over the prismatic telescope is, that it uses the full light of the whole objective at all times, while the prismatic has only half light. This objection to the latter is completely removed when the spacing target rod is used; and it is fairly well met by the employment of self-reading rods, to which reference will be made later.

In adapting the prismatic stadia telescope to the needs of the surveyor, several important matters have to be determined relating to the prism, and also to the telescope and the rod.

In my experiments I have combined a telescope of 30 diameters magnifying power with a prism of 1 foot throw to 100 feet, and also with a prism of 1 foot throw to 150 feet. I shall speak of these two combinations, respectively, as 30 d : 100 and 30 d : 150. I have also combined a telescope of 20 diameters magnifying power with three prisms throwing, respectively, 1 foot in 50 feet, 1 foot in 100 feet, and 1 foot in 150 feet; and I shall speak of these combinations as 20 d : 50; 20 d : 100; and 20 d : 150. Finally, I have combined a telescope of 10 diameters magnifying power with prisms of 1 foot to 50 and 1 foot to 100; and I shall call these combinations 10 d : 50 and 10 d : 100.

The first fact I encountered in these experiments was, that 10 d : 50 and 10 d : 100 can be used with uncorrected prisms, since the amount of color does not seriously injure the observation; while no prism not thoroughly achromatic was found satisfactory for either the 20 d or the 30 d telescope.

Fig. 2.

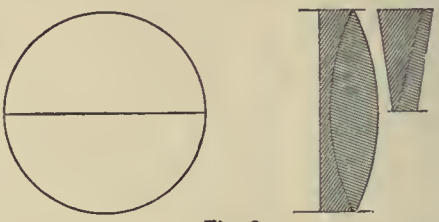


Fig. 3.

Figure 2 represents what appears to me to be the rational and proper mode of combining the prism and the objective; while Figure 3 shows an improper combination. In Figure 2 the bent rays are the exact counterpart of the direct rays, while in the adjustment of Figure 3 this is not the case.

Choice of a Prism. — For hand use a wide prism is preferred, say 1 foot displacement in 50 feet distance. For a fixed telescope 1 : 100 or 1 : 150 would be preferred, according to the distances to be sighted and the length of the rod to be used. A prism throwing 1 : 150 will permit readings at 1,500 feet on an 11-foot rod, while a 1 : 100 prism will read only to 1,000 feet on the same rod. On the other hand, where a standard steel tape with sliding targets is

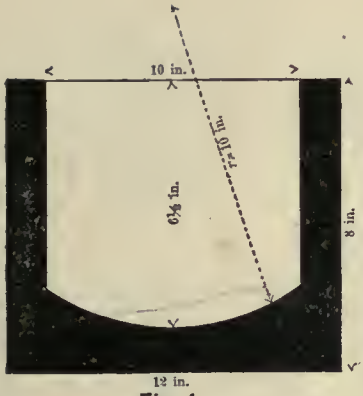


Fig. 4.

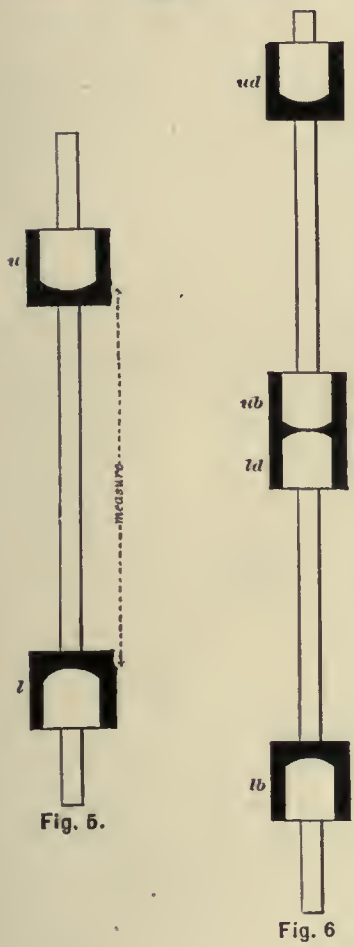


Fig. 5. Fig. 6

used instead of a rod, we may say that the wider angle prism will give more accurate work than the narrower.

Choice of a Telescope.— Since a large field is not needed, we have to deal with two considerations only: the lightness of the instrument and its power. If the former consideration governs the choice, I would recommend a light 10 *d* or a light 20 *d* telescope; if the latter, I would recommend a 20 *d* or a 30 *d* telescope.

Choice of Rods. — In this direction I believe I have settled the question as regards the choice of a spacing rod with sliding targets; but I do not feel so sure with respect to the self-reading rods.

The spacing rod represented in Figures 4 to 9 is so preëminently superior to every other which I have tried that I strongly recommend it. This target stands out strong and bright under any circumstances. It can be read against a dark background in the woods, and also against a light sky background on a hilltop.

In Figure 5 the rod is represented as seen by the eye. Figure 6 shows it as seen by the prismatic stadia telescope. In this figure *ud*, *ld* are the upper and lower images by the direct ray, and *ub*, *lb* are the upper and lower images by the bent ray. The observer, of course, neglects *ud* and *lb*, and uses for his contact *ub* and *ld*. The important advantages of this form of contact are shown in the subsequent figures.

Figure 7 represents the targets approaching contact. Figure 9 shows the images lapping, in which case there is a bright, lens-shaped image, indicating the amount of lap. Upon its appearance the assistant is signaled to draw the targets slowly apart; and the instant the white lens becomes invisible the reading, Figure 8, is taken. The disappearance of this white lens gives a close and positive reading. The results stated below were obtained with this reading.

Fig. 7.

Fig. 8.

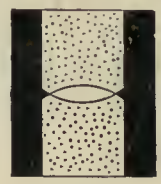


Fig. 9.

A target of the size shown in Figure 4 can be read easily at 2,000 feet distance with a 20 *d* telescope, in an even atmosphere. A much smaller target will suffice for short sights. The design of Figure 4 has more strong points in its favor than any other yet tried by me.

The spacing targets, Figures 4 to 9, may be used on a steel tape if the following conditions are observed: The tape must be held at right angles to the line of sight. If the right angle is at one of the targets, it will be found easier to reproduce than at a point half way between the targets. For distances much greater than 2,000 feet a larger target or a higher power telescope, or both, will be needed.

Figure 10 represents a tape as used by me on a 1,970-foot sight with the above targets and a 20 *d* telescope.

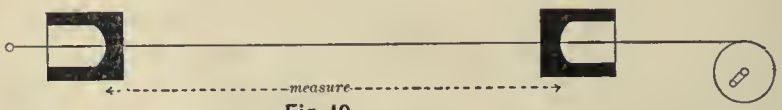


Fig. 10.

Self-reading Targets. — Three forms which have been experimented upon are represented in Figures 11, 12 and 13.

Figure 11 gives good results with a 10 *d* : 50 combination for short distances. It may be graduated, as shown, with 2-foot unit or with 5-foot or 10-foot unit. In the latter case, it would be used for long distances with a 20 *d* : 100 combination up to 30 *d* : 150 combination; but the individual feet would have to be divided by the eye. The skeleton second image on it reads 68 feet.

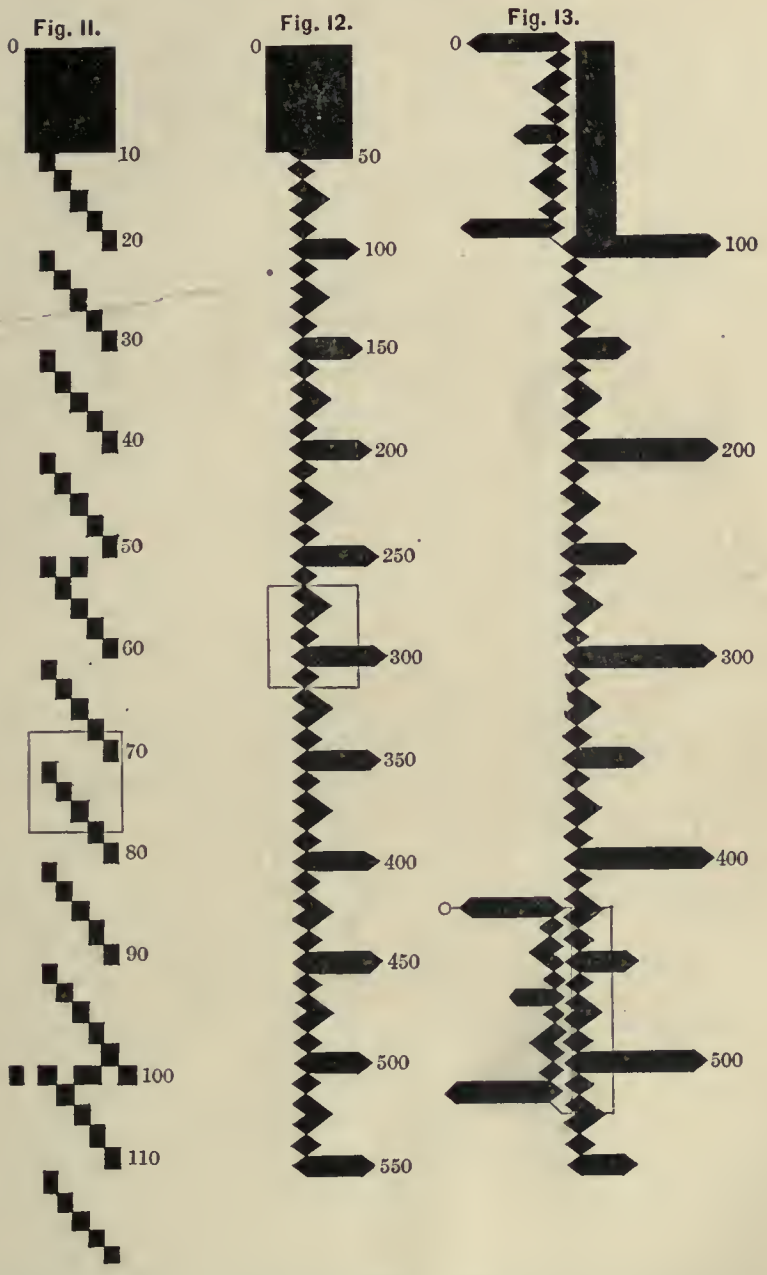


Figure 12 may be graduated with 1-foot, 2-foot, 5-foot, or 10-foot units, according as a short-distance or a long-distance rod is wanted. The skeleton reading of the second image upon it indicates 265 feet.

Possibly, for certain distances, this graduation may be found easier to read than that of Figure 11.

Figure 13 represents a target rod with an optical vernier. The space from 0 to 100 feet on the rod is graduated into eleven parts, while the spaces from 100 to 200, also from 200 to 300, and so on down, are graduated into ten parts. Moreover, the space from 0 to 100, which we may call the vernier, is graduated on the opposite side to the other readings, so that the second image can bring it down and give a vernier reading between the two images. The partial skeleton second image gives a reading of 423 feet, no dividing by the eye being required. By this system an 11-foot rod may be made for a 30 d : 100 combination, which will actually give readings of individual feet at 1,000 feet distance.

The vernier reading is the most fascinating idea I have met in my investigations. Whether it is really practical can only be decided in the field. The images which are important for the reading are half light images, and therefore dim, while with the other two forms of target a full light reading is obtained.

Limits of Error. — Partly by reason of the limited time at my command for experiments, and partly because every time I went out to get definite records of practice I made some discovery which led to an improvement in the apparatus, I can only promise at this time to give detailed figures at an early day as a supplement to this paper. The figures I am now prepared to publish were taken with a curved spacing target of the design shown in Figures 4 to 9.

With a 20 d : 150 combination, holding the rod at 100 feet distance, I obtained five separate readings of the distance in feet between the targets, which were 0.653, 0.654, 0.653, 0.653, 0.653. With a 30 d : 150 combination I made these four successive readings at 100 feet distance: 0.675, 0.675, 0.675, 0.675; all with the spacing target described in Figures 4 to 9.

If I had made an error of 0.001 foot upon the rod, this would correspond to an error of 0.15 per cent., or 0.15 foot in 100 feet. But I did not make this error in four readings with 30 d : 150, and only made it once in five successive readings by the 20 d : 150 combination. If the prism had been a 1 : 100 prism, the error referred to would have been 0.1 foot in 100. If the prism had been a 1 : 50, the error would have been .05 foot in 100 feet. I cannot imagine a reason why the percentage error at 1,000 feet or 2,000 feet, when

the atmosphere is steady, should be any greater than it is at 100 feet. From the above considerations, I believe I am safe in saying that the error of the prismatic stadia is well inside of 0.1 per cent.

I hope to present, in the near future, some figures actually obtained for both short and long distances.

If one desires to determine the distance to a point without the trouble of sending an assistant there, it may be done with a pair of Wollaston camera lucidas, as shown in Figure 14, in which W_1, W_2 are the two cameras.

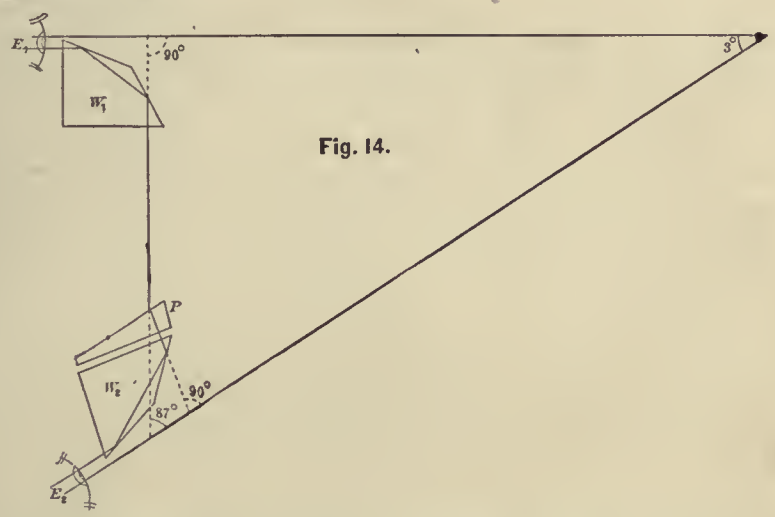


Fig. 14.

If the Wollastons both give 90°, then the requisite deviation from 90° may be obtained by inserting a prism, P, as shown. If, however, the two Wollaston angles add up to say 177° to 178°, then the apex angle will be 2° or 3°, and will answer without the addition of another prism. After the two observers, E_1, E_2, have found their places, so that their respective images coincide, then they can measure the base line between them by a prismatic stadia telescope; and knowing the factor of the Wollastons, 3 feet or 4 feet to 100, the distance to the unknown point may be determined approximately by multiplying the observed distance between E_1 and E_2 by the factor — 3 feet to 100, 30 feet to 1,000, 300 feet to 10,000, and so on.

In making my designs and in testing my instruments I have been helped materially by Captain A. H. Russell and Messrs. J. Hays Gardiner, W. H. Weston, Franklin Knight, Luis T. Verges and W. S. Hutchinson, friends to whom I wish to make acknowledgment.

MAGNETIC CONCENTRATION OF IRON ORE.

BY HARVEY S. CHASE, S.B.

Read March 24, 1892.

THERE are in the records of the Patent Office drawings and descriptions of separating machines, whose letters patent have now expired, but which many years ago were fitted with permanent magnets or battery-driven electro-magnets, and did excellent work, as models, upon magnetic iron sands or clean magnetic ores. It is only within the past three years, however, that magnetic concentration has been practically established, and separating plants built which handle very large amounts of crude, low grade ore. Of such plants, fitted to handle from one hundred to one thousand tons of ore daily, there are now many; but before describing them it will be worth while to consider upon what material magnetic concentration must work, and then to examine the locations of the magnetite bearing strata, and the present market for concentrates made from such ores.

Magnetic oxide of iron, Fe_3O_4, occurs in nature in black crystals, which are naturally magnetic, and which vary greatly in size and are associated with widely differing minerals. These associated minerals in any ore form the so-called "gangue," and upon its constitution, as well as upon the size of the crystals of magnetite, depends the success of magnetic separation or concentration.

The problem of separation, as defined today, consists in handling quickly and cheaply large amounts of low grade ores, delivering this material (from which the best grade is often "cobbed," or picked by hand) to large jaw crushers; then to crushing rolls or pulverizing mills, which break it to uniform sizes, after careful screening and recrushing of the particles refused by the screens. From two to six or more passes are commonly required to gradually granulate the ore to the fineness desired, which is determined by the relative sizes of the magnetite crystals and those of the constituents of the gangue. The sizing commonly made in practice runs not finer than twenty "mesh,"

although in special cases reaching even fifty "mesh," or a condition much like coarse flour. This degree of fineness is, however, extreme; and while excellent concentrates of this minuteness have been made and sold, and used successfully in blast furnaces, yet it is very doubtful if there can ever be proper pecuniary return for concentrates made from material which requires such excessive grinding.

Between four and sixteen "mesh" is found the usual limit of granulation for nearly all ores from which concentrates can be made profitably today.

The Appalachian mountain system, composed in considerable part of the crystalline Archæan rocks which were the first to raise their heads above the great primeval ocean, contains throughout its length great veins or leads of the black oxide or magnetic oxide of iron, running mainly along the eastern slopes from the north side of the Great Lakes in Canada, through eastern New York, along Lake Champlain, and inward among the Adirondacks (famous for their magnetic ore, from which charcoal blooms are still made for horseshoe nails and other specialties, whose quality has never been rivaled); then crossing the southeastern portion of New York, through northeastern New Jersey, southeastern Pennsylvania, through Maryland, along the upper reaches of the Shenandoah and the James, and the Holston in Virginia, down into eastern Tennessee, western North Carolina, and northern Georgia. In many portions of this great stretch of a thousand miles this ore has been found in large quantities and of great purity, but throughout the far larger portion of the distance the oxide is so disseminated through the rocks that the percentage of metallic iron is too low to allow of its use in modern blast furnaces, so long as high grade Lake Superior or other hematites can be obtained even at thrice the cost per ton.

Along this line are planted the anthracite and charcoal furnaces, which before the advent of Bessemer were accustomed to use the ordinary ores, making ordinary foundry and forge irons. Today many of these furnaces are closed, and many others are running with little or no margin of profit, on account of the competition of modern, well-equipped furnaces located near the finest coke or near the magnificent hematites of the Lake Superior districts. The cost of transportation is heavily against these old eastern furnaces, inasmuch as not only coke but ore, has now to be brought across the mountains, distances of from 300 to 1,000 miles; so that, in spite of the nearness of the

great markets for pig iron and the relatively short haul to the seaboard, the district east of the Alleghenies has shown for many years that apparently its day for profitably making pig iron is past. But now magnetic concentration has stepped to the front, and after long and very costly experimentation during the past ten years, is prepared to furnish to these eastern furnaces an ore which is excellent for blast furnace use, and is especially well fitted for the needs of the direct process steel-maker, and which is easily and very cheaply produced from waste dumps and from mines long ago abandoned (because of the low percentage of iron, or high phosphorus, or sulphur, or other deleterious ingredients), and situated oftentimes within a few miles of the furnaces.

For instance, there is in New Jersey a mine, belonging to a well-known iron company, which has been worked steadily for very many years, shipping 60 per cent. of the amount mined as ore, which runs about 55 per cent. of metallic iron, while the remaining 40 per cent. of the output, containing from 35 to 40 per cent. of metallic iron, has been thrown upon the waste dump or stored in the worked-out rooms in the mine. It is now found that this refuse is exceedingly well adapted for concentration, and that by establishing a plant which will cost not over thirty-five thousand dollars, an output of about 100 tons of concentrates per day, running 65 per cent. of iron and low in phosphorus, will be obtained, and the resulting profit from the sale or use of these concentrates is sufficient to much more than return the cost of the plant each year; or, to put it differently, to pay a dividend of 4 per cent. and over upon the total capital stock of the company, and all from what has long been considered worthless refuse.

There are many mines in New Jersey, New York, and Pennsylvania which are today in just this condition, and which have been waiting the appearance of a practical, cheap, and durable magnetic separator, which can be set up and run day and night, if need be, without special electrical attendance, and the repairs to which can be confined to the smallest proportions, and made quickly by common, unskilled labor.

The problem of magnetic separation of iron ores as found practically in the mines is a very different one from the separation of a purely magnetic substance from a purely non-magnetic one, because the crystals of magnetite scattered through the ore-bearing rock are

of very different sizes. The average may be a certain definite size, and the crushing and screens adapted thereto; but there will be a large percentage of very fine crystals which, in the crushing for the average, will not be broken apart from the gangue, and if separated into "heads" will carry gangue with them, and if separated into "tails" will carry too much iron into the "tails," and cause loss. There are two ways of solving the problem, one of which is to crush all the material to the size of the finest particles; which means, of course, a large crushing plant, heavy wear and tear and excessive cost, sufficient often to prohibit the process. This is the common method of treatment, however, and many thousands of dollars have been expended upon separators and upon crushing machinery as thus applied. The more modern method is to crush coarsely at first, and separate upon a machine which has a power of selection, so that there will be first thrown off a "tails" containing practically no magnetite; next a second "tails," or middlings, with some iron in the mixed particles; next a third, and fourth, or other division, and finally a practically pure magnetite as "heads." The first "tails" are thrown away, the "heads" are sent to the bins or cars, and the second and third or other "tails" are treated again, and such portions of them as may be necessary crushed sufficiently fine to break apart mechanically the magnetite from the gangue. The crushing of three fourths of the product is thus saved, and the cost and wear and tear correspondingly reduced, and the problem is efficiently and economically solved.

The machine shown is one intended for this purpose, and as it is of my own design I prefer to let it speak for itself, and to test it with various kinds and sizes of crushed ores.

The machine consists, first, of a spirally wound magnetic wheel, giving a very strong magnetic field, about its whole circumferential area, and owing to the method of winding it is equally efficient whatever its length, though the regular sizes of machine vary by one-foot divisions, and determine the width and capacity of the separator.

The crushed ore is fed upon the top of this wheel by a patent feed, and is carried by a belt of cotton duck around through the magnetic field downwards. The absolutely non-magnetic particles are thrown off at once by centrifugal action, while all particles wholly or partially composed of magnetite are carried to the under side of the wheel, from which the belt passes nearly horizontally under a

peculiarly made magnet with "consequent poles" of varying strength and distance from the belt, causing a rotative and tumbling action in all these particles as they pass through successive magnetic fields, while held up close to the belt by the magnetic attraction, directly against the influence of gravity. The coarser mixed particles fall out first, then the finer, and finally the "fines" or "slimes," and are caught in separate receptacles.

The pure, or nearly pure, magnetite traverses the whole length of the under side of the belt, usually from three to four feet, and at the end is attracted by a magnetic wheel, similar to the first one and four inches in diameter, around which the belt takes a quarter turn upward for twelve inches, and then passes over a first pulley and under a second, and returns horizontally above the magnets to the first wheel. By the attraction of this second magnetic wheel the magnetite particles are whisked around the corner, while the fine dust continues in a tangential line horizontally. At this point a current of air strikes the belt and assists to drive off this dust, or in the wet process a current of water acts in the same way. The magnetite, now going upward, passes out of the field of the second magnetic wheel, and is immediately drawn off from this belt by a third magnetic wheel, over which runs a second belt to a pulley at any desired distance. The concentrates are thus loaded on this second belt and conveyed to any point desired and dropped into bins or cars, the intermediate products being treated again as described before. The wet process machine varies from the dry only by a water-tight tank, in which all the moving parts are set beneath the surface of the water, the "tails" and intermediate products being taken out from the bottom of this tank by ordinary wooden scrapers at an angle, while the "heads" go off upon the second belt out and above the surface.

Certain ores work much better wet, and others dry; and this separator has a marked advantage, as by drawing off the water from the tank the wet machine becomes at once a dry machine, and works equally well as such.

More rapid work can be done dry, but much cleaner and more perfect work, wet; and the repairs and renewals are much less in the wet process, as the water acts as a lubricator as well as a cleansing medium.

The machine shown was about five feet in height and four feet by three in floor space, fitted with motor, rheostat, am-meter, fuses,

switches, etc., complete in itself, so that only the wires of a hundred and ten volt circuit had to be connected. Tests were made upon Lake Champlain magnetic sands, upon Witherbees, Sherman and Company's high phosphorus and low phosphorus ores, upon waste from the dump of the Hibernia mines, New Jersey, and from the Weldon mines (a very coarse ore) in the same State. The products were collected in separate boxes, and tested by the members present with strong horseshoe magnets. In every case the "tails" showed no appreciable iron, scarcely a tenth of one per cent., while the "heads" ran above 68 per cent. of iron. The other products included the "fines," which carried a trifle of iron in minute division, while the mixed particles showed upon examination that recrushing was essential to break the fine-grained magnetite from the coarser gangue.

A DESCRIPTION OF SOME REPEATED STRESS EXPERIMENTS.

By JEROME SONDERICKER, C.E.

Read April 14, 1892.

A LARGE proportion of the fractures occurring in the ordinary use of structures and machinery is the result of repeated stresses. For the determination of the capacity of material to resist such stresses the ordinary tests of strength are inadequate. Wöhler's noted experiments brought this matter prominently before the engineering profession, and his law is now quite generally recognized. This law is as follows: "The fracture of material can be effected by variations of stress repeated a great number of times, of which none reaches the breaking limit. The differences of the stresses which limit the variations of stress determine the breaking strength. The absolute magnitude of the limiting stresses is only so far of influence as, with an increasing stress, the differences which bring about fracture grow less."

In order to use to the best advantage in design the results of repeated stress experiments, it is important to understand the causes which lead to fracture in structures under such stresses. Aside from the influence of the quality of the material, two explanations suggest themselves: First, that fracture results from molecular changes produced by the repeated stress, extending through the entire mass, and altering the physical properties of the material in such a manner that it becomes less and less capable of supporting its load; second, that the fracture is the outcome of a gradually extending crack; such cracks starting at weak places in the material, such as flaws, surface indentations, and the like; or at places where excessive stress is caused by more or less abrupt change of form, as at shoulders, keyways, angles, etc. In brief, we conceive the cause of failure to be local, the material as a whole suffering no deterioration.

In investigating the cause of failure under repeated stresses, it is important to make a study of the elastic changes produced by such

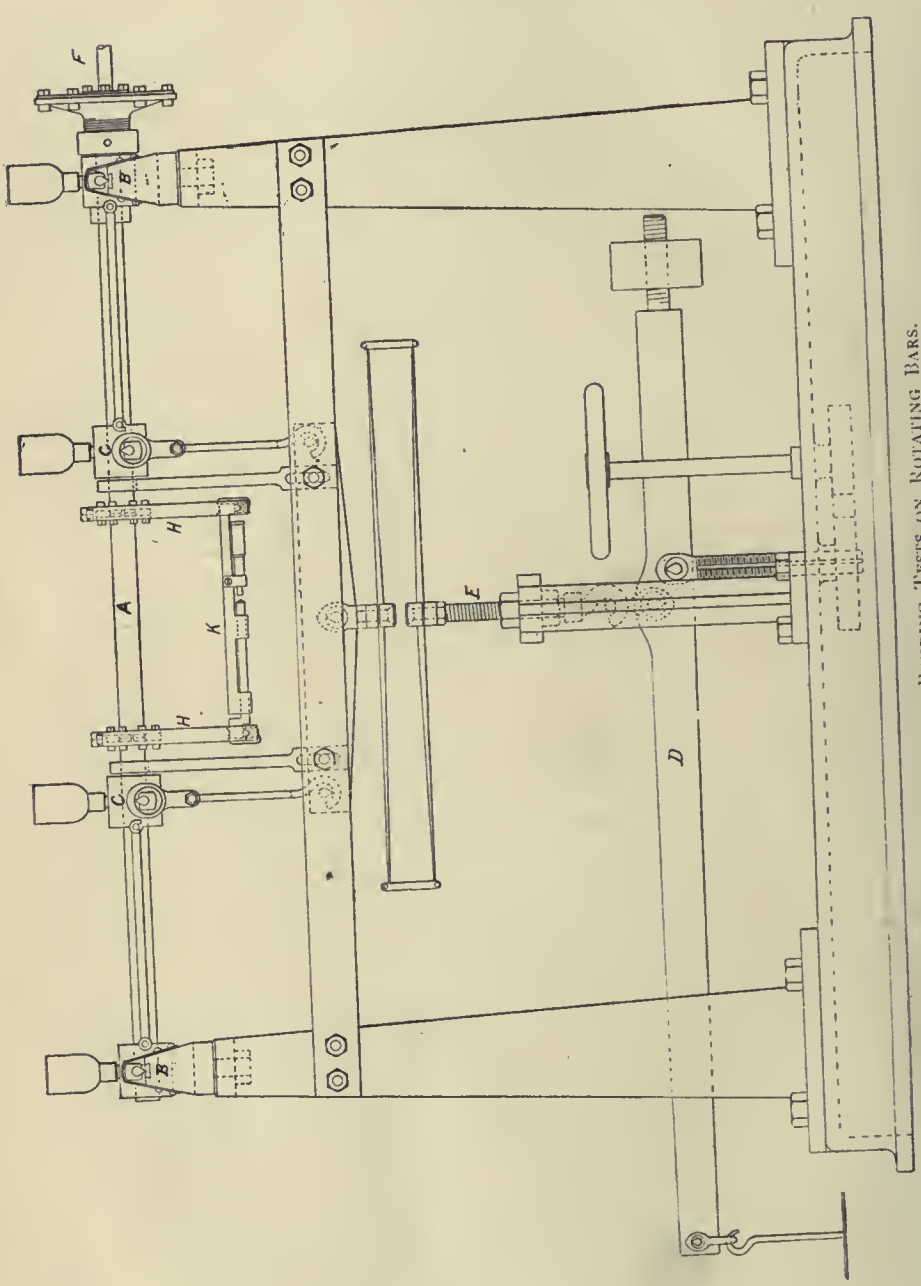
stresses. We have as yet very little information in this direction. The most noteworthy experiments are those of Bauschinger, published in *Mittheilung* xv, 1886. These experiments were performed on bars subjected to repeated tension stresses. In addition to these we have a series of experiments on rotating bars, made at the Watertown Arsenal, and published in the United States Government *Reports of Tests of Metals made at Watertown Arsenal*, for 1888 and 1889.

About one and one half years ago the writer began a series of experiments at the Institute for the purpose of making a special study of the effect of repeated stresses upon the elasticity of iron and steel; and although at present the tests are only fairly begun, it is the purpose of this paper to present the results thus far reached. The tests were made upon bars loaded transversely while rotating as a shaft; thus the material was subjected to alternate tension and compression stresses of equal magnitude.

The machine used in making these tests is shown in the figure on the next page.

The test shaft A is supported at B, B, and loaded equally at the two points C, C, which are equidistant from the supports. The load is applied by means of the lever D, acting through the spring, cross-bar and links shown in the drawing. While the bar is rotating the rod E is clamped to the standard, the spring maintaining the load on the specimen; thus any variation of stress due to the vibration of the lever is avoided. From the manner of loading, the portion of the shaft between the points C, C is subjected to a uniform bending moment; and hence the shaft, if of uniform diameter, is equally stressed at all sections between these points and curves in the arc of a circle. One end of the test shaft is connected by a flexible coupling F with the driving shaft. The power is furnished by an electric motor, running day and night. The speed of the test shaft in all the tests thus far made was about five hundred revolutions per minute. An automatic device is employed for stopping the motor when the shaft cracks, but before it has broken completely in two. The halves are afterwards pulled apart in the tension machine, in order to examine the fractures.

The apparatus for measuring the elasticity of the test bars is as follows: Two arms H, H are clamped to the central portion of the shaft, at right angles to its length and 10 inches apart. A measuring apparatus K, consisting of a micrometer fixed to one of two



MACHINE FOR REPEATED BENDING TESTS ON ROTATING BARS.

sliding bars, is supported by these arms at points 5 inches from the axis of the shaft, spherical bearings being used. When the shaft deflects, the arms, pointing downwards, spread; the measurement taken being the distance that the two points 5 inches from the axis of the shaft move apart. These distances are for brevity referred to as deflections. The measurements were taken to $\frac{1}{10,000}$ inch, this corresponding to a deflection in the 10-inch length of about $\frac{1}{10,000}$ inch, and to an extreme fiber strain of about $\frac{1}{1,000,000}$. The arms remain attached to the shaft, revolving with it.

During the tests the machine is stopped at intervals of from one to four days, and a set of measurements taken, in order to determine what elastic changes have occurred. The quantities observed are total deflection, elastic deflection and set; the set being the deflection remaining after the load is removed, and the elastic deflection the difference between the total deflection and set. The order of procedure to be adopted in making the deflection measurements is a matter of the first importance. The minute sets occurring within and in the immediate vicinity of the elastic limit conduct themselves as if they constituted a sort of backlash or lost motion between the particles of the body; so, in order to obtain comparable results, we must follow a certain order in taking the observations, and adhere strictly to this throughout. Thus, if the machine be stopped with the measuring arms pointing downwards, then the load be removed, after which a series of observations is taken, first applying the load again to obtain the total deflection, then removing it to obtain the elastic deflection, the difference being the set, it will be found that this difference is nearly or quite zero; the re-application of the load causing no further permanent deflection. If, on the other hand, the shaft be stopped with the arms pointing upwards, then the load be removed and a similar set of observations taken after rotating the shaft 180°, a decided set will be obtained. This, moreover, will be twice what would have been obtained if the load had been removed while the shaft was rotating. In all the experiments the order of observations was as follows: (1) Stop the shaft with the measuring arms pointing upwards; (2) remove the load, then rotate the shaft 180°; (3) insert the measuring apparatus and take a reading; (4) apply the load and take a second reading; (5) remove the load and take a third reading. The difference between the first and second readings is the total deflection; between the second and

third readings, the elastic deflection; between the first and third readings, the set. The separate observations were taken at three-minute intervals, and were corrected for change of temperature in the test bar. This correction rarely amounted to more than one or two ten thousandths of an inch.

Thus far sixteen specimens have been tested, including wrought iron, open-hearth and Bessemer steel, and cold rolled shafting. The duration of the individual tests varied from fifteen hours to two months, the corresponding number of revolutions being about one half million and forty millions. The test bars are one inch in diameter, the cold rolled samples being tested in the condition in which they were received; the others were turned and polished. Some of these latter were of uniform diameter, the central portion of others being slightly reduced in section. All the bars of each set were of the same quality, in most cases being cut from one rod. The table on pages 79 and 80 gives a summary of the tests. Elastic changes occurred in the wrought-iron bars, and in the steel bars of sets 4 and 5. The changes were the same in character for all the bars of any one material, but differed for the different materials. The elastic deflections remained very nearly constant in all the tests, the changes occurring mainly in the set. The diagrams on page 78 represent these changes. The abrupt changes in the diagrams are due to changing the load on the shaft. The numbers on the diagrams indicate the fiber stress in pounds per square inch under which the shaft was rotating, as will be seen by reference to the table. The horizontal lines marked O are the lines from which the sets are laid off as ordinates. Each vertical space represents .001 inch micrometer reading, the corresponding deflection being about one fourth of this, as previously stated.

In the wrought-iron shaft, Diagram 1, the set under a stress of 24,000 pounds per square inch at first increased rapidly for a short time; after which it decreased, rapidly at first, then more and more slowly; the final set being less than one half that observed at first. When the stress was increased successively to 25,000 and then 26,000 pounds, no further changes of consequence occurred; but under a stress of 27,000 pounds changes occurred similar in nature to those observed at first, the final set under a stress of 27,000 pounds being less than the original set at 24,000 pounds. When a stress of 28,000 pounds was applied the shaft quickly broke.

The changes in the bar of soft steel, Diagram 2, are very similar in nature to those just described. The initial increase in set is wanting, the change consisting in a decrease of set, rapid at first, proceeding more and more slowly, and practically ceasing after about five million revolutions. The final set under 36,000 pounds is considerably less than the initial set at 32,000 pounds. The original elastic limit of the steel was 28,000 pounds per square inch.

In Diagram 3 we have represented changes exactly opposite in nature to those already described. Changes first occurred at a stress of 34,000 pounds, the elastic limit of the steel being 50,000 pounds. The change consists in a continuous increase of set, rapid at first, proceeding more and more slowly, finally ceasing. When the stress is increased successively to 36,000 pounds and 38,000 pounds, the resulting changes are similar, but more marked, the final set being .0118 inches, or over 4 per cent. of the total deflection. This shaft is still under test, and has already withstood a total of about nine million revolutions under this large set.

With a different form of diagram in each case, we may expect that other varieties of iron and steel might give still different diagrams, so only a partial discussion of them will be attempted.

1. In each case the changes are rapid at first, but proceed more and more slowly until the bar finally settles into a stable elastic condition. We conclude that elastic changes once begun do not continue indefinitely, and hence that it cannot be inferred that such changes will necessarily finally result in fracture.

2. In Diagrams 1 and 2 the changes were first apparent at the elastic limit of the material; and, with the exception of the initial increase of set in Diagram 1, these two diagrams represent the same general law of change; namely, a continuous decrease in the amount of set, rapid at first, proceeding more and more slowly, and finally ceasing. This continuous decrease of set harmonizes with the well-known effect of cold working of ductile metals, which is to diminish their plasticity and raise their elastic limit and breaking strength. Thus it would seem probable that in these two cases the diagrams represent the normal effect of the cold working beyond the elastic limit to which the bars were subjected.

3. The third diagram is so different from the first and second that we are led to believe that the changes produced in this steel by the repeated stress are of a different nature from those produced in

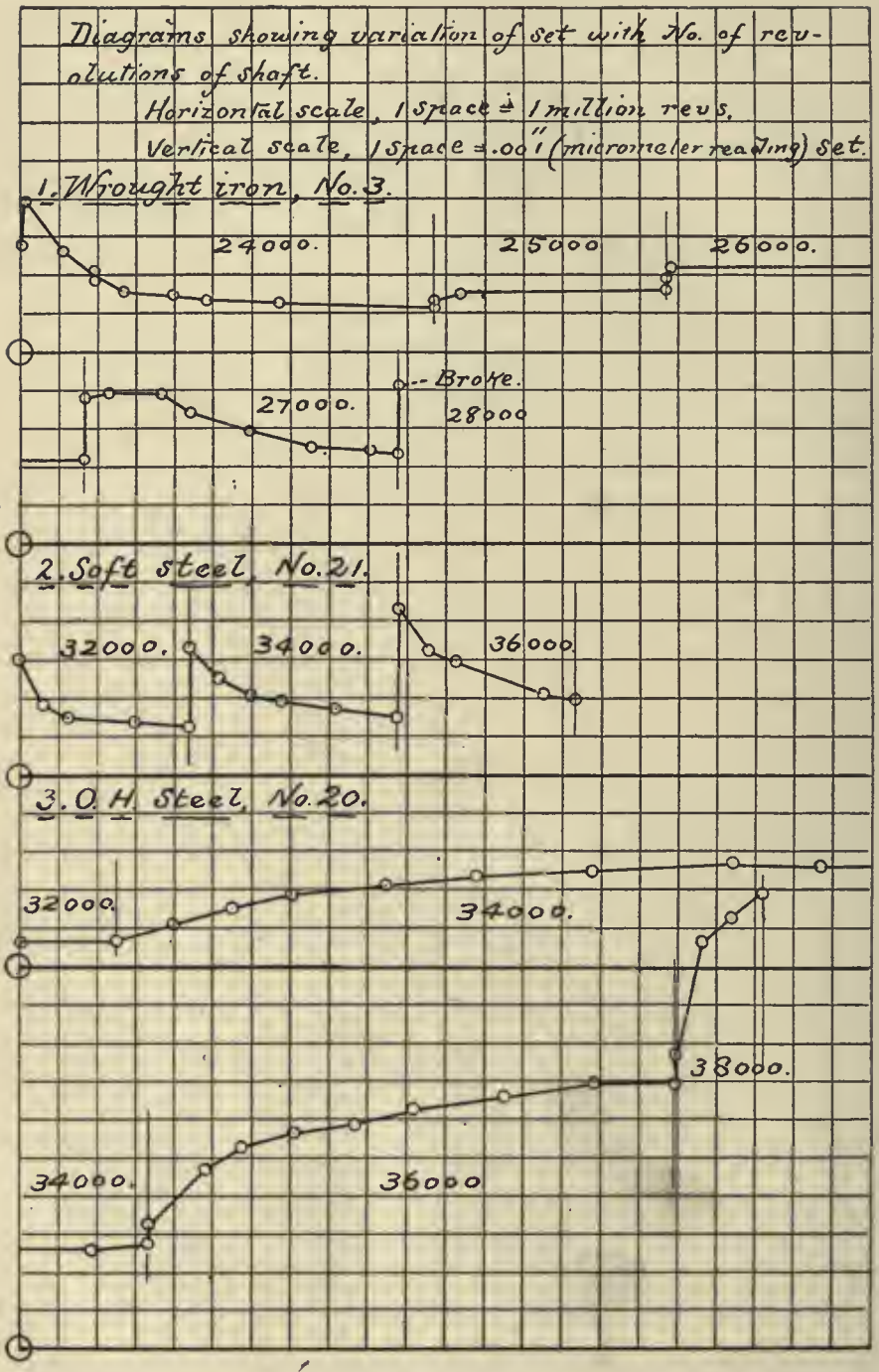
the other two cases. These changes occurred within the elastic limit, and the direction of the change is the reverse of the other two. The open-hearth steel had originally an elastic limit of 50,000 pounds per square inch. Considerable elastic change, however, occurred at a stress of 34,000 pounds, the set increasing from .0007 inches to .0028 inches; and under a stress of 38,000 pounds a set of .0118 inches, or over 4 per cent. of the total deflection, was reached. The soft steel had originally an elastic limit of 28,000 pounds per square inch. At the end of the test this steel had a set of .0019 inches as contrasted with .0070 inches in the open-hearth steel under the same stress; and it was at the end of the test more nearly perfectly elastic at 36,000 pounds than the open-hearth steel at 34,000 pounds. Whatever be the explanation of these opposite results effected in these two cases by the repeated stress, Diagram 3 proves conclusively that elastic changes may occur at stresses within the elastic limit.

4. The elastic limit does not appear to be a sufficient criterion for judging of the capacity of material to resist repeated stress. We have seen that in two cases the metal has successfully withstood stresses extending beyond the elastic limit; in the third, elastic changes tending to increase the set have occurred far within this point, and a large proportion of the test bars broke within the elastic limit without any elastic changes occurring.

5. In these tests, since the stress was uniform over a length of about twelve inches, favorable opportunity offered for observing the influence of local conditions in causing fracture. Of the fourteen bars thus far broken, two broke at little loose splinters on the surface, two at slight indentations caused by careless handling, three broke where the measuring arms were attached, two at one of the shoulders, and one at a mark burned around the shaft by the electric current; thus only four broke without any local cause being apparent. In the turned steel bars, when a cylindrical form was used, the shafts broke where the measuring arms were attached. In order to prevent this the central portion of the bar between the arms was slightly reduced in section, using carefully rounded shoulders; then fracture occurred at the shoulders. It is thus seen that the fracture can, in ten out of the fourteen bars broken, be assigned to local conditions. Again, it is improbable that changes of structure would occur without accompanying elastic changes; so we can confidently conclude that no change of structure occurred in the eight bars

which broke without elastic changes being produced by the repeated stress, and that in all these cases the fracture was the result of local conditions. It thus appears from these experiments that, in the majority of cases at least, fracture under repeated stress is not due to physical changes affecting the whole mass of the metal, but rather to such local conditions as were referred to at the beginning of this paper.

In conclusion, while no direct connection between the minute elastic changes treated in this paper and the resistance of the material to repeated stress has yet been established, it is evident that observation of these changes may be of assistance in discussing the results of repeated stress experiments; and the writer would urge the importance of making such observations in connection with experiments of this nature.


Diagrams showing variation of set with No. of revolutions of shaft.
Horizontal scale, 1 space = 1 million revs.
Vertical scale, 1 space = .001" (micrometer reading) set.
1. Wrought iron, No. 3.
24000.
25000
26000.
-- Broke.
27000.
28000
2. Soft steel, No. 21.
32000.
34000.
36000
3. O. H. Steel, No. 20.
32000.
34000.
38000.
34000.
36000

SUMMARY OF REPEATED STRESS TESTS.

1. WROUGHT IRON.

Elastic limit = 24,000 pounds per square inch. Tensile strength = 51,000 pounds per square inch. Elongation = 26% in 10 inches.

No. of Specimen.	Stress Lbs. per sq. inch.	No. of Revolutions.	Elastic Change.	Remarks.
1	15,000	5,220,000	No.	
. . .	26,000	2,285,000	Yes.	Broke 3½″ from center.
2	20,000	5,402,000	No.	
. . .	32,000	486,000	Yes.	Broke at center, at mark made by electric current.
3	24,000	10,727,000	Yes.	
. . .	25,000	8,962,000	No.	
. . .	26,000	3,931,000	No.	
. . .	27,000	8,155,000	Yes.	
. . .	28,000	589,000	Yes.	Broke at shoulder.
4	28,000	2,506,000	Yes.	Broke 1″ from center.

2. COLD ROLLED STEEL SHAFTING.

Elastic limit = 58,700 pounds per square inch. Tensile strength = 81,700 pounds per square inch. Elongation = 9½ % in 6 inches.

No. of Specimen.	Stress Lbs. per sq. inch.	No. of Revolutions.	Elastic Change.	Remarks.
9	36,000	968,000	No.	Broke near box.
10	32,000	1,410,000	No.	Broke.
11	26,830	5,941,000	No.	Broke inside box, at splinter.
12	24,000	10,577,000	No.	
. . .	26,000	10,071,000	No.	
. . .	27,000	5,285,000	No.	
. . .	28,000	4,701,000	No.	
. . .	29,000	5,235,000	No.	
. . .	30,000	4,522,000	No.	
. . .	32,000	1,066,000	No.	Broke at splinter.

3. BESSEMER STEEL.

Elastic limit = 41,600 pounds per square inch. Tensile strength = 68,100 pounds per square inch. Elongation = 26.4 % in 10 inches.

No. of Specimen.	Stress Lbs. per sq. inch.	No. of Revolutions.	Elastic Change.	Remarks.
13	36,000	1,271,000	No.	Broke where clamp was attached.
14	36,000	1,805,000	No.	Broke where clamp was attached.
15	36,000	2,489,000	No.	Broke in slight indentation.

4. OPEN-HEARTH STEEL.

Elastic limit = 50,000 pounds per square inch. Tensile strength = 81,000 pounds per square inch. Elongation = 22 % in 10 inches.

No. of Specimen.	Stress Lbs. per sq. inch.	No. of Revolutions.	Elastic Change.	Remarks.
17	32,000	1,352,000	No.	Broke in slight indentation.
18	32,000	11,370,000	Not observed.	
. . .	34,000	5,266,000	Not observed.	
. . .	36,000	6,982,000	Yes.	Broke at dent made by clamp.
19	36,000	7,686,000	Yes.	Broke at shoulder.
20	32,000	2,527,000	No.	
. . .	34,000	22,816,000	Yes.	
. . .	36,000	13,576,000	Yes.	
. . .	38,000	2,263,000	Yes.	Not broken.

5. SOFT STEEL.

Elastic limit = 28,000 pounds per square inch. Tensile strength = 62,600 pounds per square inch. Elongation = 26.4 % in 10 inches.

No. of Specimen.	Stress Lbs. per sq. inch.	No. of Revolutions.	Elastic Change.	Remarks.
21	32,000	4,400,000	Yes.	
. . .	34,000	5,300,000	Yes.	
. . .	36,000	4,700,000	Yes.	Not broken.

THE THEORY AND PRACTICE OF TANNING.

By WALTER J. SALOMON.

Read May 26, 1892.

I FEEL indeed highly honored to have been called upon to speak before your society as a representative of an industry which has been known from time immemorial, is to-day one of the most powerful industries of the world, though of a modest bearing, and will be connected with mankind as long as human beings inhabit this earth. Leather manufacture and its collateral branches take fourth place in the list of manufactured products of the world, iron, wool and cotton alone taking precedence. The dressing of leather extends centuries and centuries back. Herodotus, the historian, speaks of the Libyan women wearing goatskins stripped of hair and covered with vermilion. The Bible mentions the leathern girdles of Elijah and John the Baptist. In the Mosaic account of the tabernacle we find reference to the rams' skins dyed red; while the Gordian knot, made of leathern thongs, was cut about 330 B.C. On the doors of the Rochester (England) Cathedral, there were numerous skins of pirates; and so we can quote illustration after illustration showing the great age of leather manufacture in different shapes and forms. But we shall turn aside from retrospection and view the leather of to-day, its mode of manufacture, the results obtained and the why and wherefore of tanning.

First, what is leather? Leather is the result of the half chemical, half mechanical combination of the albuminous hide fiber and a substance which preserves the hide or skin and renders it pliable, thereby making it useful in the arts and manufactures. A great many different kinds of hides and skins are used in the manufacture of leather — cowhides, calfskins, horsehides, goatskins, sheepskins, kangaroo, alligator, lizard skins, etc. These hides or skins are purchased in different states; some are dry, some green — that is, just as they are taken from the animal — and some in the green salted state to prevent decomposition in shipping.

Right here it will be interesting to become acquainted with the structure of the hides and skins used by tanners. The hide may be

divided into three parts; namely, the epidermis, the coriin or cutis — or better still, the actual leather hide — and the lower flesh structure. The epidermis is totally removed in the work of preparing the hide for the actual tanning process; and the lower flesh structure is, as a rule, also removed little by little at different stages until we have the leather ready for the market composed wholly of the coriin or cutis. The study of the formation and situation of hide fiber with the aid of the microscope is of great interest and a source of pleasure to the student. It is a remarkable fact that, as we divide or class the living animals according to their nature, in an exactly coinciding way can we class the same animals according to their hide fiber structure. Just as we class the alligator, snake and lizard in the reptiles on account of their similar mode of living, appearance, etc., so we find that they resemble each other even more in their fiber structure. The fiber of these reptiles is so much alike that it is almost impossible to distinguish any difference under the microscope. Again, the structures of the cowhide, horsehide, calfskin, deerskin, etc., resemble each other greatly, being of a looser nature than the fiber of reptiles. But in all hide structure we have the same method of growth and trend as in that of the development of the bark of trees. The lower flesh structure is always of a loose nature, consisting, more or less, of loose bundles of fiber interwoven. As these bundles enter the cutis or coriin they become more compact, the weaving becoming of a closer nature; and as they near the grain or papillar the bundles disappear, and the single fibers are woven in a mass that becomes more solid, until it reaches the compactness of a horny grain which withstands the wear to which the leather is subjected. Just as the bark on trees is worked off and replaced by younger layers from the wood, so is the grain on the living animal continually replaced by fiber from the lower portions of the hide.

Leather is commonly divided into two classes; namely, sole and upper. We will first treat of upper leather. When the hides are received in the tannery they are soaked in water; this soaking removes all blood, salt and lymph substance, and softens the flesh still remaining on the hide so that it may be easily worked off with a knife. After removing the superfluous flesh the hides are resoaked for a period of two days, as a rule, and then placed in the lime. The lime is simply a solution of calcium hydrate or ordinary quicklime in water, and, like all alkalies, has the property of dissolving albuminous substance

and distending fibrous substance. Upon this property is based the process of liming hides, as the action of the lime is a solvent one throughout, resulting in the loosening of the hair roots and the removal of the epidermis. The lime actually divides the fibers into numerous smaller fibers, a fact which is absolutely proven by the microscope. All of this work is done in the department called the "beam house;" so called, evidently, from most of the work being done on beams. It will be seen that the tendency throughout all this work has been to cleanse the hide and hide fiber, and to remove all unnecessary flesh substance, leaving naught but the real hide substance or coriin. Heavy hides are usually limed from four to five days; but light skins of which pliability and elasticity are required are limed from eight to fourteen days. Glove leather is sometimes limed from twenty to thirty days.

Now comes an important step; this is the bating process. Before the hides are bated they are washed or rinsed well in water, whereby almost all of the lime is removed. The bate consists of manure, or if the raw material is a light skin — goatskin, for instance — fermented bran. The manures used are chicken, pigeon and dog. In the first place the effect of the manure bate is to dissolve and remove all the unnecessary interfibrous substances, leaving room for a freedom of motion of the fiber, and facilitating the entrance of the tannin molecule. The effective principles of the manure bate are pepsin and pancreatin, substances which are formed in the body of the animal. While in Vienna last year I had occasion to make a comparative analysis of the strength of the various manures used by tanners, and I found that, taking chicken manure as 1, pigeon manure was 2, and dog, $2\frac{1}{2}$. The hide is put in a warm mixture of manure and water, and digested, as it were, until the hide is found to be "low" enough, or, in other words, digested to such an extent that a prolongation of the process would damage the grain or surface of the hide. This action of the bate may be prettily illustrated by taking an unclean piece of wet linen, halving it, washing one half and hanging both pieces out to dry. Upon examination the unclean piece will be found to have dried stiff and the washed piece to be very much softer. This is due to the untrammeled freedom of motion of the clean fiber. It has been very generally supposed that the purpose of the bate is to remove the lime, but as a rule the lime has been removed in the previous rinsings. Should small quantities of lime remain in the hide

they may be dissolved out in the bate, as sulphuretted hydrogen is usually present and transforms the lime into sulphide of calcium, which is easily soluble in water. The time of bating varies from four hours to a day and night.

Now, as to the bran bate and drench. The bran is usually set with warm water a day or two before it becomes necessary to use it. This brings about fermentation, in the course of which lactic, acetic and some butyric acids are formed. The amount of acid varies from $\frac{1}{2}$ per cent. to $1\frac{1}{4}$ per cent. These acids will dissolve or remove lime, although lactic acid only suspends it; but the effective principle is not based upon this, but upon the plumping and distention of the fiber, thereby again preparing the skin for the actual tanning process. In fact, the test of the tanner is to make a sort of small balloon of the skin and press the inclosed air out, the ease with which this is done being used as a guide to judge the arrival at the wished-for result; namely, a porous, plump skin. An important factor in both of these bating systems is the action of those infinitely small animalculæ, of which we know much, but actually very little — the bacteria. The effect of bacteria on the whole process of tanning is, in fact, of great importance, and furnishes a field for not only very interesting study, but also of practical research, with profitable results to the successful student. In the bates the actions of the bacteria are absolutely uncontrollable, and this is the reason why this stage is so very difficult and requires constant watchfulness. The least change of temperature has an effect. For instance, a bate is placed at say 100° Fahrenheit in July. Now, in the ordinary course of events, the day temperature would be between 80° and 100°, and we might expect at night a temperature of about 70°. But should the night, instead of being cool, be close and warm, say about 80° to 95°, this would, firstly, increase the working powers of the pepsin, as the bate would not cool so quickly, and the greater the temperature the greater the dissolving power; secondly, the bacteria would propagate themselves at an enormously high rate, as a temperature of 90° to 100° is the ideal one for the propagation of these animalculæ. After the leather has been digested to an extent which is most satisfactory for that purpose it is intended to be used for, it is removed from the bate and "slated;" that is, the hide is worked out with a slate set in a wooden handle. This removes all the so-called "impurities," which are really the interfibrous substances in solution and suspension. The hide is then ready to begin the process of actual tanning.

Now, as to sole leather. Hides suitable for this kind of leather are only the heavy cowhides and steer and bull hides. The hides are soaked in the same way; but many, instead of being limed, are "sweated" to remove the hair. Sole leather must be of a firm nature; therefore it is not necessary to have freedom of motion of the fiber, and consequently liming is done away with. The sweating is accomplished in a tight room which is full of vapor; this softens the hair roots, and within four or five days it is possible to brush all the hair off. Again, bating is unnecessary, and they are simply "wheeled" in water and then are ready to tan.

Right here it may be interesting to discuss the tanning materials in use throughout the world. Nature has again been great in her wisdom, for look where you will, from the heart of the United States or Europe to the interior of Africa, you will find substances containing available tannin in quantity. In the United States our main tanning materials are hemlock bark and oak barks, although other materials are used, which come almost totally from foreign countries. England produces only oak bark; France, oak bark and pine bark; Germany, the same; Austria-Hungary, the same with the addition of knoppern, a small excrescence on branches, the result of the sting of an insect; Asia Minor, galls; India, catechu; Indo-China, gambier; Australia, mimosa bark, etc. There are hundreds of other tanning materials, but I do not wish to weary you by going into further details on this subject. The tannin contained varies from 7 to 70 per cent., and each and every one has its distinct color effect. Some tannins color red, others yellowish; again, some give a purple tint, others brown, etc. Some barks make a firm leather, some a loose, pliable stock; and as there is a continual international traffic in these materials, it has become the task of a good tanner to produce the best results in color and weight and firmness with all of them at his disposal.

We now arrive at a step of great interest. This is the actual process of tanning, in its practical as well as its theoretical phase. By careful study and continual examination of specimens with the aid of the microscope, the following conclusion as to the manner of tanning a hide and the entrance of tannin into the fiber is reached. The tannin molecules in solution in the bark liquor attack the hide from both sides, and first tan the outside fibers; then they are redissolved and go on to the next inner layers, their places being taken by other molecules. Now they leave the second fibers and go to the third,

being again replaced, and so on, until the opposite armies of molecules meet, when the hide is tanned. The outside fibers are consequently tanned and retanned thousands of times. There is a condition which a tanner calls "dead tan," and this is based upon the total filling up of all the interfibrous canals, glands, etc. Tanners whose object it is to produce good-weight results usually try to reach this "dead-tan" point as nearly as possible and practicable. This theory may seem a little odd at first, but, as I said before, continual observation leads us to this conclusion.

Now, as to the practical side of this process. The bark or tanning material is placed in a wooden tank, or "leach," as the tanners call it, and water being pumped on it, a decoction is made. This gives us the so-called bark liquor, and in this liquor the hide is tanned. It takes all the way from six days to nine months to tan ordinary hides and skins, while a walrus hide requires a time of three years; this variation being caused by the different purposes for which leather is used, and the different peculiarities and qualities consequently necessary. In the case of light skins, by keeping them continually in motion by the aid of the paddle wheel, rockers, etc., so as to admit of the easy entrance of tannin, we are enabled to tan much more quickly; but these skins are used for light shoes and other light work for which no excessive firmness or solidity of the leather is needed. Heavy leather is, as a rule, "struck through," that is, colored through with tannin, and then placed in strong liquors or laid away in a pit and strewn with bark to tan for weight.

Of late years quite an industry has sprung up in the manufacture of extracts of bark for the benefit of tanners far away from bark regions, or to insure the tanner against a possible shortage of bark. The extracts are made by boiling the bark or tanning material with water in the open air or in a vacuum, and then boiling down to a thick mass containing about four to six times the strength of an equal quantity of bark. This, of course, is of great assistance to the tanner. But the correct use of these extracts is not quite as well understood by some tanners as might be wished.

There are also a number of other materials besides bark used for preserving leather. One might arrange tanning materials in a row in the order of their tanning ability, with tannins at one extremity, coloring matters in the middle, and aniline, which also preserves leather, as the least effective. It is a peculiarity of all metals of the third

chemical group, namely, iron, chromium, aluminum, zinc, manganese — but nickel and cobalt excepted — that they have the property of entering into a half chemical combination with the fiber by the power called absorption, and thereby so enveloping the fiber that it becomes imputrescible. Alum tanning in different forms and processes has been in use for centuries, but chrome tanning, which is, as a rule, brought about by a soluble chrome salt being soaked into the skin and then precipitated on the fiber by reduction, has been in practice only since the year 1856. This tannage, as it is called, is being used to a large extent at present in the manufacture of glazed kid. Another tannage is made through the dongola process, which is a combination of the mineral tannage, the tannin tannage and the addition of oil or grease, usually in a saponified form. Again, we have the oil tannage for chamois and buckskin. Oil tanning is done by working the skins from the bate, usually bran, in oil, and removing the superfluous oil by soda or potash, or other alkali.

There has been much discussion within the last few years as to the real value of electric tanning, its real effect on the hide fiber and its value in assisting and facilitating the tanning of leather. So far there has been no success made of this especial branch, although it has been tried in numerous places. A Parisian firm has been making numerous experiments for the last three or four years, but as far as I have been able to learn, has met with dismal failure. The idea that electricity can have any effect on tannin in any other way than to precipitate the phlobaphenes, that is, undesirable coloring matters, seems to me extremely doubtful. I have seen a number of experiments made with the aid of the different poles used, such as platinum, tin, gold and copper, the peculiarity being that as the poles differed the color differed, one giving a bright and beautiful color, others giving darker colors. This may be of value to some tanners of upper leather or sole leather who desire to obtain excellent color results. That the electricity has any effect upon the hide fiber is exceedingly doubtful; and as to its quickening the tanning, it cannot do so except indirectly by precipitating the undesirable coloring matter, thus leaving an easily soluble tannin which will pass into the hide at a quicker rate. But it requires constant care and study in order to obtain satisfactory results. Possibly new things may develop which may be of value, but for the present we can hardly call it an important phase of the leather industry.

There are thousands of ways of finishing leather after it leaves the tan, so that it will be impossible for us to go into this step any further than to say that most of the upper leather is colored black, worked out well to flatten it, and after being oiled is placed on the market.

It often seems strange to a person not connected with the leather trade that enough hides and skins can be procured to meet the demand for the many purposes that leather is used for; but instead of a shortage we have at present a large quantity in the market for which buyers are hard to find. The American leather trade is increasing yearly. Formerly we imported large quantities of leather, but to-day our import list has dwindled down to almost nothing, while we in turn are doing a very large export trade.

Before closing I would like to remark that all of the before mentioned theories have been made from actual observation in practice, not, as has been so often the case, by making theories and fitting practice to them.

The present opportunity to discuss the establishment of a technical school for the tanning industry is so good that I will pause a few moments on this subject. In Europe, and especially in this case Austria, they have carried the plans and ideas of technical education to a very successful point. You will find there schools and institutions of all special lines of industry; among them, most elaborate schools of forestry, inclusive of bark culture and economical production, and a most successful tanning school. The advantages of this technical education undoubtedly are apparent to this society. The leather industry of this country has reached a point where it must avail itself of the glorious advantages that the correct and conscientious application of science is capable of giving it. The associated manufacturers have appointed a committee to further this plan of the establishment of a school, but this committee have not met with the success in a pecuniary way that they hoped for. Now, it may be possible that such an annex or department of this Institute could be founded here in the heart of the leather country, and the possibilities of the plan may furnish food for reflection for your society.

AN INDEX TO THE LITERATURE OF ANGELIC AND TIGLIC ACIDS FROM 1842 TO 1892.

By HENRY P. TALBOT, Ph.D.

Among the large number of organic compounds which have excited the interest of chemical investigators during the last half century, few have done so to such a degree as have angelic and tiglic acids. This may largely be attributed to the fact that these compounds have, until recently, resisted all attempts to discover their molecular structure or the exact relation in which they stand to each other. Even to-day these problems have not been wholly solved; for while it is generally conceded that these two acids present a well-marked instance of geometrical isomerism, it cannot be asserted that this fact is decided beyond question; nor has the geometric structure of either acid been so positively determined as to preclude the necessity for further research.

The number of investigators who have made these compounds the subject of their researches is large, and the literature relating to them which has accumulated during fifty years is considerable in amount. It has seemed to the writer that a compilation of this literature, scattered as it is through many journals and written in several languages, would be an assistance to future workers in this field.

The labor attending the search for information relating to angelic acid is materially increased by the confusion of names in the earlier literature, the name angelic acid having been applied to compounds whose properties do not correspond to those of the acid which now bears that name. In the accompanying index such cases are indicated by interrogation marks placed against the name angelic acid.

It is also necessary to bear in mind, when studying the literature of tiglic acid, that for a number of years that acid was only known as methylcrotonic acid, the name tiglic acid not appearing until 1870.

The history of these interesting compounds is outlined in the following:

Angelic acid has been longer known than tiglic acid. It was first isolated by Buchner (1842) from the roots of the *angelica archangelica*, and received from him the name which it now bears. Buchner describes this new acid with some detail, but views it more from the standpoint of the pharmacist than that of the chemist, and makes no hypothesis as to its structure.[1]

Meyer and Zenner, in 1845, and Gerhardt, in 1848, both published analyses of angelic acid which correspond to the empirical formula $C_5H_8O_2$; but they, again, offered no suggestion as to the probable molecular structure of the compound.

The work of Frankland and Duppa, published in 1865, describes the first of the attempts to determine upon a structural symbol for angelic acid, and also begins the history of tiglic acid under its structural name, methylcrotonic acid. By the action of phosphorus trichloride upon ethyl metho-ethoxalate they prepared an acid, the properties of which they described (and which were later found to correspond with those of tiglic acid), and to which they gave the name methylcrotonic acid. From the origin and method of preparation of this compound, its structure should correspond to the symbol $CH_3-CH=CCH_3-COOH$.

Frankland and Duppa found it to be isomeric with angelic acid; and that, like the latter, when treated with potassium hydrate, it yielded acetic and propionic acids. This would, according to their views, limit the possible symbols for angelic and tiglic acids to the following:

$$\begin{matrix}CH_3\\CH_3\end{matrix}\!>\!C=CH-COOH, \text{ and } CH_3-CH=CCH_3-COOH.$$

Of these the latter is the probable symbol of methylcrotonic acid, as has been shown, which leaves the first as the only possible symbol for angelic acid, unless, as they said, "we assume some unknown cause for isomerism."

Between 1865 and 1870 but little work was done upon these acids. Jaffé acted upon angelic acid with bromine, and made a partial study of the addition-products; but no important theoretical deductions were

[1] In an abstract of a recent article by Kondakoff [Ber. 25 (1892), 34 Ref.], the original of which is published in the J. Russ. Chem. Soc., the statement is made that Buchner regarded angelic acid as α-ethylacrylic acid. The writer has been unable to confirm this statement.

made from the results of his work. It may be remarked here that both angelic and tiglic acids have been found in other natural products than *angelica*, notably in the oil of camomile. References to all of this work are to be found in the index, although not all of it is mentioned in this historical sketch.

In 1870 Geuther and Fröhlich prepared an acid from croton oil to which they gave the name tiglic acid, at the same time calling attention to its isomerism with the methylcrotonic acid of Frankland and Duppa, and suggesting the possible identity of the two compounds. This question of identity was not, however, settled until 1878, in which year Schmidt and Berendes established it beyond question. From that year the name tiglic acid has been more generally used than methylcrotonic acid.

Additional and convincing proof of the structure of this compound was furnished by Wislicenus and Rohrbeck (1887), who prepared it synthetically by the withdrawal of water from α-methyl-β-oxybutyric acid.

In 1877 Demarçay investigated the bromine addition-products from angelic acid, and from the results of his work was disposed to represent the structure of the angelic acid by the symbol

$$\begin{array}{c} CH_3-CH-CH-COOH. \\ \diagdown \quad \diagup \\ CH_2 \end{array}$$

The arguments which Demarçay brings forward to support his symbol are not convincing.

Schmidt and Sachtleben, in 1878, were the next to attack the problem. They assumed the correctness of the symbol proposed by Frankland and Duppa, and attempted the synthesis of angelic acid from isobutylformic acid by oxidation and subsequent withdrawal of one molecule of water. In this they were unsuccessful, and they were also unable to offer any new suggestions as a result of their labors. Indeed, it may be stated that no attempts to prepare angelic acid synthetically have met with any success. Acids which have been prepared synthetically and reported as angelic acid have subsequently been found to differ from that acid in physical properties and in chemical behavior. In several instances the acid was dimethylacrylic acid.

Fittig and Kopp discovered, in 1876, that both angelic and tiglic acids were present in the oil of camomile; and called attention to the

fact that the earlier workers, who obtained their angelic acid from that source, may have dealt with an impure article. With the hope of being able to throw new light upon the structure of angelic acid, Fittig and Pagenstecher (1879) investigated the dibromides and hydrobromides of this and of tiglic acid. They were unable to detect either physical or chemical differences between the corresponding compounds of the two acids.

Fittig, when commenting upon this work, pointed out that the identity of the hydrobromides may be explained by assigning to angelic acid the symbol

$$CH_3-CH_2-\overset{CH_2}{\overset{//}{C}}-COOH.$$

But the assumption is there made that when tiglic acid is acted upon by hydrobromic acid the bromine atom takes the *a*-position as regards the carboxyl group, which has since been disproved. Moreover, the symbol failed to explain the identity of the dibromides, and was, therefore, not more satisfactory than those which had already been proposed.

Schmidt, in 1881, was the next to contribute to this subject. He showed that both angelic and tiglic acids yield the same reduction-products upon treatment with hydriodic acid, namely, methylethylacetic acid. He did, however, note a difference in the hydro-iodides formed by the moderate action of hydriodic acid upon the two unsaturated acids, although he did not investigate these hydro-iodides fully. This may be cited as the first instance in which different products had been obtained by the action of reagents upon the organic compounds in question.

Schmidt proposed as a possible symbol for angelic acid, the following :

$$CH_2=CH-\overset{CH_3}{\overset{/}{C}}H-COOH.$$

This formula is, however, at variance with the fact that acetic and propionic acids are formed by oxidation with potassium hydrate — a fact which has been well established. Fittig also pointed out later (1882) that neither the symbol proposed by Schmidt nor that proposed by him (as already given) can represent the structure of the molecule. He confirmed the statement made by Schmidt that the hydro-iodides of the two acids differ as regards physical properties, but stated that they yielded identically the same products upon treat-

ment with sodium carbonate and that this product was a pseudo-butylene.

Assuming the correctness of Schmidt's symbol, the hydro-iodide formed would correspond to the symbol $CH_2I-CH_2-CHCH_3-COOH$, and the hydro-iodides from Fittig's symbol would be represented by

$$CH_3-\underset{\underset{COOH}{|}}{CI}-CH_2-CH_3 \quad \text{or} \quad CH_2I-\underset{\underset{COOH.}{|}}{CH}-CH_2-CH_3$$

Of these, the first should yield a valero-lactone upon treatment with water; the second, upon treatment with sodium carbonate, should yield an oxy-acid; and the third a butylene, but not the pseudobutylene obtained from tiglic acid hydro-iodide, which has the structural symbol $CH_3-CH=CH-CH_3$. Since, according to Fittig, the butylenes formed from the two hydro-iodides were identical, none of these symbols could be correct.

The action of potassium permanganate upon tiglic and angelic acids has been investigated by Beilstein and Wiegand in 1884, and by Kondakoff in 1888; but the results of their work simply confirmed the structural similarity of the two acids, no difference in their behavior having been detected.

The outline already given presents briefly the results of the investigations carried on before 1887, at which time Wislicenus turned his attention to the solution of the problem which these two isomeric compounds offered. In 1874 van't Hoff and Le Bel presented to the scientific world their theory regarding the arrangement of the atoms within the molecules of organic compounds. Wislicenus had, as van't Hoff states, by his remarks upon the isomerism of the lactic acids, furnished the incentive to further thought which finally developed into the latter's theory of three-dimension formulas. Van't Hoff himself suggested that his theory would furnish a means of explaining many cases of isomerism which the previously accepted hypotheses were unable to deal with. It was Wislicenus who, later on, developed the theory of van't Hoff, and afterward perfected the methods for the treatment and detection of geometrically isomeric bodies which have been of great value in the study of this class of compounds.

In 1887 Wislicenus, basing his assertion upon the behavior of crotonic and isocrotonic acids, stated his belief that angelic and tiglic acids would be found to be geometric isomers. In 1888 Wislicenus and

Pückert re-examined the dibromides of these acids, which Fittig and Pagenstecher had pronounced identical, and found that the dibromides did not yield the same pseudobutylenes upon treatment with sodium carbonate, but that the hydrocarbons formed were geometric isomers. From a line of argument given in detail in the original article [see reference (60) in the index], based upon the results of Pückert's work, Wislicenus assigns to angelic acid the symbol

$$\begin{array}{l} CH_3-C-H \\ CH_3-\overset{\|}{C}-COOH, \end{array}$$

and to tiglic acid the symbol

$$\begin{array}{l} H-C-CH_3 \\ CH_3-\overset{\|}{C}-COOH. \end{array}$$

These symbols are also in accordance with those assigned to isocrotonic and crotonic acids, the latter of which corresponds in its behavior to tiglic acid, and received the symbol

$$\begin{array}{l} H-C-CH_3 \\ H-\overset{\|}{C}-COOH. \end{array}$$

To Wislicenus, then, is due the credit for the first experimental evidence in favor of geometrical isomerism as an explanation of the behavior of the two acids under consideration.

It is interesting to note that Ostwald's deductions from a series of determinations of the affinity constants of these acids, made in 1889, support the views held by Wislicenus. Ostwald found that tiglic acid is the weaker of the two acids, which corresponds to the symbol already given, in which the methyl group attached to the upper carbon atom is nearer the carboxyl group than in angelic acid. The methyl group is generally more positive than the hydrogen atom alone, and exerts a greater influence upon the carboxyl group. This theory is also supported by the crotonic and isocrotonic acids, the former (corresponding to tiglic acid) being the weaker of the two.

Additional support has been given to the assumption of Wislicenus that these acids furnish an excellent example of geometrical isomerism by the work of Mellikoff and Petrenko-Kritschenko (1890), who have formed and isolated four chlor-oxy-valerianic acids resulting from the action of hypochlorous acid upon the double-bonded organic acids, in exact accordance with the theory.

The writer also made a more thorough study of the hydro-iodides of these acids than that made by Schmidt or Fittig, to which reference has already been made. That the hydro-iodide from angelic acid is a distinctly different body from the tiglic acid hydro-iodide there can be no question, and a study of these compounds with a view to the confirmation of the theory of Wislicenus regarding the constitution of the original acids has developed nothing which is at variance with the formulas given.

On the other hand, it must be stated that the correctness of the work of Wislicenus and Pückert has been seriously questioned by Fittig (1891), who reaffirms the validity of his own work with Pagenstecher. The work of Pückert has also been indirectly attacked by Favorsky and Debout, and the latter criticism also affects the work of the writer, and has hindered the publication of the theoretical deductions except as referred to in reference (80) of the index.

Notwithstanding these criticisms of the work done in Wislicenus' laboratory to prove the geometric isomerism between these two acids, the fact remains that the symbols proposed by Wislicenus are the only ones which do not conflict with observed facts. The weight of the evidence is strongly in favor of this theory, and it is to be hoped and expected that new proofs of this isomerism will soon be furnished.

Finally, a few words in explanation of the accompanying index are necessary.

The following journals were consulted from their beginnings to April, 1892, inclusive, except as otherwise indicated, or unless publication of the journal ceased before 1892:

American Chemical Journal; Annalen der Chemie; Annales de Chimie et de Physique (1822–1883); Annalen der Physik (Poggendorff) (1822–1877); Archiv der Pharmacie; Berichte der deutschen chemischen Gesellschaft; Bulletin de la Société chimique de Paris; Chemisches Central-Blatt; Comptes rendus de l'Académie des Sciences; Gazzetta chimica italiana; Journal of the Chemical Society; Journal für praktische Chemie; Jahresbericht der Chemie; Jahresbericht der reinen Chemie; Monatshefte für Chemie; Recueil des Travaux Chimiques des Pays-Bas; Répertoire de chimie pure et appliqué; Zeitschrift für Chemie; Zeitschrift für physikalische Chemie.

In addition to the above the following text-books have been consulted, although it has not been thought best to include the page references in the index: Beilstein (second edition, 1890), Gmelin, Gerhardt, Kekulé, Kolbe-Von Meyer (Graham-Otto), Roscoe and Schorlemmer, and the dictionaries of Fehling, Watts and Wurtz.

The abbreviations used are those recommended by the Committee on the Indexing of Chemical Literature of the American Association for the Advancement of Science.

Each journal was consulted by means of the general indexes for a series of years, where these existed, otherwise through the indexes for each year or volume. It is to be remarked that in some cases, notably those of the *Archiv der Pharmacie* and the *Comptes rendus*, the general indexes are of such a character that, unless the acids in question form a part of a title or occupy a prominent place in any article, it is impossible to discover that they are mentioned at all.

It is probable that a few references of minor importance may have been overlooked, but it is believed that the index is practically complete to April, 1892, as regards the journals and text-books mentioned.

The references marked by an asterisk were not accessible to the writer, and are classified wholly from the information obtained from abstracts of their contents given in other journals. In each case the original article is the first reference given, and all references following the words "in abstract" are less complete than the original article. No attempt has been made to make the list of abstracts wholly complete, but in most cases in which these abstracts occur in several languages one reference in each language has been given. The references to the *Jahresbericht der Chemie* generally furnish a full record of the journals in which the given article has appeared, and these references are given before those of the abstracts.

The arrangement of the index of subjects is that which suggested itself as convenient, and requires no additional explanation. The order of the references under each division is chronological.

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   Sigil (77).
8. Crude Wood Spirit.
   Angelic acid. [?]
   Krämer and Grodzki (41).
9. Veratrum.
   Tiglic (methylcrotonic) acid.
   Bosetti (5), Ahrens (1), Stransky (79); but questioned by Wright and Luff (84).
10. Cevadin.
    Tiglic (methylcrotonic) acid.
    Wright and Luff (84).

II. SYNTHETICAL METHODS OF PREPARATION.

Tiglic (methylcrotonic) acid.

1. From ethyl metho-ethoxalate.
   Frankland and Duppa (22), Schmidt and Berendes (73), (74), von Miller (49), (50), (52).
2. From α-methyl-β-oxybutyric acid.
   Wislicenus (83), Rohrbeck (64), von Miller (50), (52), Rücker (65).
3. From the oxidation of oxyvalerianic acid.
   von Miller (49), (52); acid regarded as an isomer of methylcrotonic acid (50), (52); recognized as dimethylacrylic acid (51), (52). Compare also Duvillier (15), (16).
4. From α-hydroxyvalerianic acid.
   von Miller (54).
5. From tiglic aldehyde from Guajol.
   Herzig (27).
6. From tiglic aldehyde.
   Lieben and Zeisel (43).
7. Attempt to obtain tiglic acid by the oxidation of methylethylacetic acid.
   von Miller (53).

Angelic acid. [?]

1. Supposed formation by the oxidation of valerianic acid.
   Neubauer (55). Compare von Miller.
2. Supposed formation from ethyl acetylisopropylacetate.
   Demarçay (12); subsequent denial of the statement (14).
3. Supposed formation from oxyvalerianic acid.
   Semljanizin and Saytzeff (76). (Probably dimethylacrylic acid.)
4. Attempt to obtain angelic acid from isobutylformic acid.
   Schmidt and Sachtleben (75).

Action of Reagents.

I. ACTION OF BROMINE.

Upon angelic acid.
Cahours (7), Jaffé (31), (32), Kekulé (33).
Upon tiglic acid.
Schmidt and Berendes (73), (74).
Upon angelic and tiglic acids.
Demarçay (11), (13), Schmidt and Berendes (73), (74), Isenbeck (30), Pagenstecher (58), Schmidt (69), (70), Pückert† (60), Fittig (20).

II. ACTION OF HYDROBROMIC ACID.

Upon angelic and tiglic acids.
Isenbeck (30), Pagenstecher (57), Talbot (80), (81).

III. ACTION OF HYDRIODIC ACID.

Upon angelic acid.
Ascher (2), Jaffé (31).
Upon tiglic (methylcrotonic) acid.
Schmidt and Berendes (73), (74).
Upon angelic and tiglic acids.
Schmidt (69), (70), Fittig (19), Talbot (80), (81).

IV. ACTION OF HYPOCHLOROUS ACID.

Upon angelic acid.
Mellikoff (45).
Upon tiglic acid.
Mellikoff (44).
Upon angelic and tiglic acids.
Mellikoff and Petrenko-Kritschenko (46), (47).

V. ACTION OF NASCENT HYDROGEN.

Upon angelic acid.
Kekulé (33).
Upon tiglic (methylcrotonic) acid.
Schmidt and Berendes (73), (74).

VI. ACTION OF POTASSIUM HYDRATE.

Upon angelic acid.
Chiozza (8), (9).
Upon tiglic (methylcrotonic) acid.
Frankland and Duppa (22), Paterno (59), Demarçay (11), Schmidt and Berendes (73), (74).

VII. ACTION OF POTASSIUM PERMANGANATE.

Upon angelic and tiglic acids.
Beilstein and Wiegand (3), Kondakoff (37), (38).

VIII. ACTION OF ALKALI SULPHITES.

Upon angelic and tiglic acids.
Beilstein and Wiegand (4).

† In this connection compare also an article by Favorsky and Debout, J. prakt. Chem. **42** (1890), 149, on the brom-pseudobutylenes.

Conversion of Angelic into Tiglic Acid.

By long standing.
Schmidt (69), (70); statement afterward denied (71).
With the aid of heat.
Demarçay (13), Kopp (36).

Crystallographic Measurements.††

Angelic acid.
Schimper (66).
Tiglic acid.
Haushofer (26).

Determinations of Affinity Constants.

Of angelic and tiglic acids.
Ostwald (56).

Theoretical Considerations regarding the Constitution of the Acids.

Frankland and Duppa (22), Demarçay (11), Schmidt and Berendes (74), Schmidt and Sachtleben (75), Fittig (18), (19), Schmidt (70), Beilstein and Wiegand (3), Wislicenus (84), Ostwald (56), Mellikoff and Petrenko-Kritschenko (46), Talbot (80).

Massachusetts Institute of Technology,
May, 1892.

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†† Compare also Fock (21) and Talbot (81) for measurements of the crystals of the hydro-iodides of angelic and tiglic acids.

PROPOSED NEW FORMULA FOR YELLOW PINE POSTS.[1]

By JAMES H. STANWOOD, S.B, Instructor in Civil Engineering.

Received June 8, 1892.

Believing that the formula of C. Shaler Smith, given in Trautwine, gives too large values for yellow pine posts, and having the usual objection to the "step" formula, the writer, for his own information and use, has taken the trouble to plot the values of all the tests on *full size* sticks made at the Watertown Arsenal and printed in the government reports.

From the drawing herewith the straight line formula

$$S = 5{,}000 - 43\tfrac{1}{3}\frac{l}{d}$$

was easily deduced, which represents the average of all the tests.

For the average of the *lowest* values in each set of tests the formula

$$S = 4{,}250 - 43\tfrac{1}{3}\frac{l}{d}$$

was found. The line representing this formula chanced to be parallel to the first one. With this latter equation as a basis, the working formula

$$S = 1{,}000 - 10\frac{l}{d}$$

gives a factor of safety of 4.25 approximately. This very simple equation is easily memorized, and by its use the safe load for a yellow pine post may almost instantly be worked out "in the head."

The coefficient of $\frac{l}{d}$ being about one one-hundredth of the first term, any factor other than 4.25 may as readily be used; thus

$S = 800 - 8\frac{l}{d}$ for a factor of 5 approximately.

$S = 1{,}400 - 14\frac{l}{d}$ for a factor of 3 approximately, etc.

For sake of comparison, the formulæ of C. Shaler Smith and Mr. Ely have been plotted.

[1] Copyright, 1892.

For those who prefer not to depart from the strict column formula of the form

$$S = \frac{k}{1 + \frac{1}{a}\frac{l^2}{d^2}}$$

the following:

$$S = \frac{4.000}{1 + \frac{1}{2,500}\frac{l^2}{d^2}}$$

may be used, which, if plotted, will be found to represent the average of the lowest values.

In each of the above formulæ

l = length in inches.
d = smaller diameter in inches.

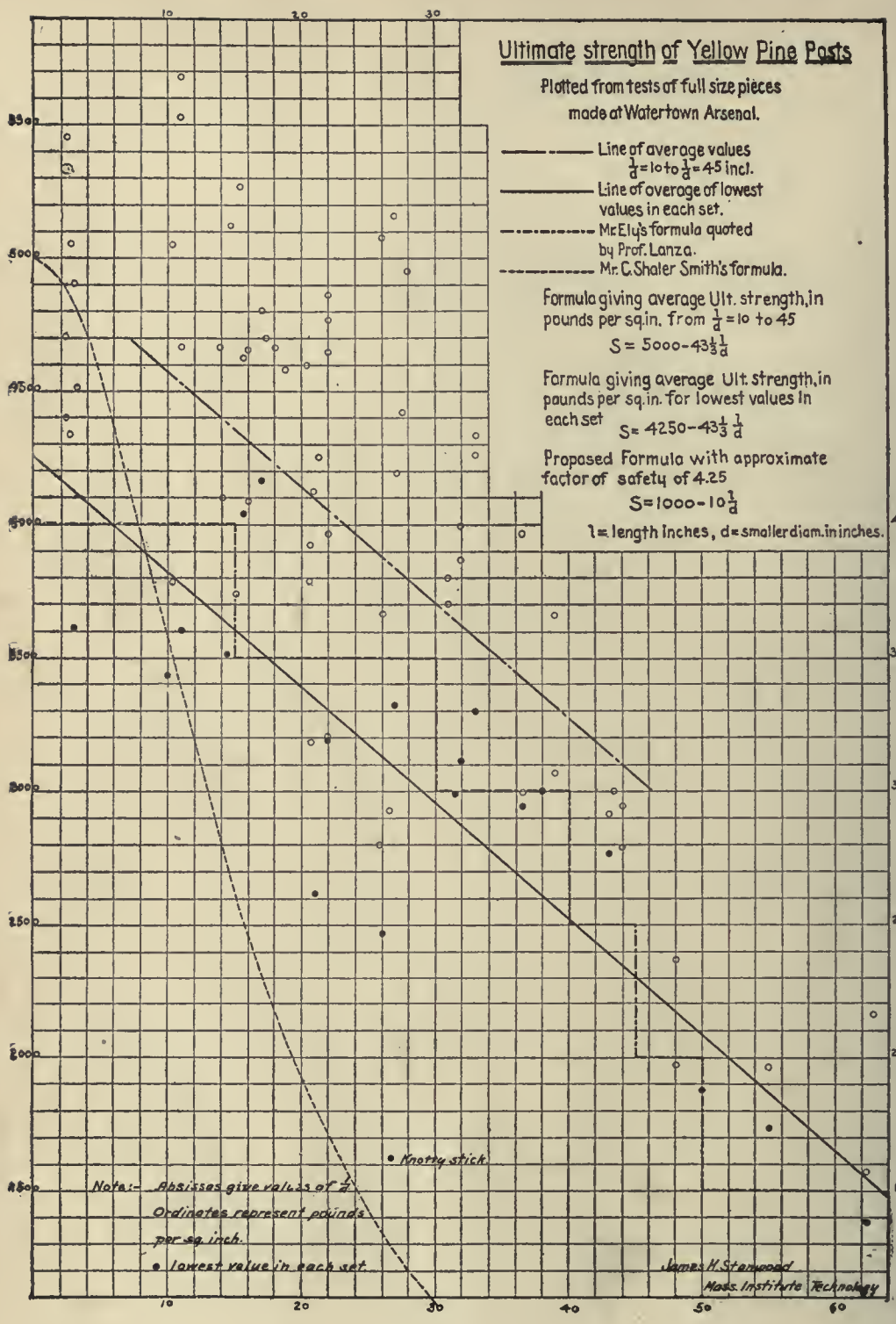
From the expression

$$S = 1,000 - 10\frac{l}{d}$$

the table of safe loads has been computed.

Trusting that this will be of use to others, it is respectfully submitted to the profession.

Since making this drawing, the recent experiments of Kirkaldy on wood columns of fairly large size have been applied to the proposed formula, and found to agree quite well with it.


Ultimate strength of Yellow Pine Posts
Plotted from tests of full size pieces made at Watertown Arsenal.
Line of average values $\frac{l}{d}$=10 to $\frac{l}{d}$=45 incl.
Line of overage of lowest values in each set.
Mr. Ely's formula quoted by Prof. Lanza.
Mr. C. Shaler Smith's formula.
Formula giving average Ult. strength, in pounds per sq. in. from $\frac{l}{d}$=10 to 45
$S = 5000 - 43\frac{1}{3}\frac{l}{d}$
Formula giving average Ult. strength, in pounds per sq. in. for lowest values in each set
$S = 4250 - 43\frac{1}{3}\frac{l}{d}$
Proposed Formula with approximate factor of safety of 4.25
$S = 1000 - 10\frac{l}{d}$
l = length inches, d = smaller diam. in inches.
Knotty stick
Note:- Abscisses give values of $\frac{l}{d}$
Ordinates represent pounds per sq. inch.
• lowest value in each set
James H. Stenwood
Mass. Institute Technology

Safe Loads, in Pounds, for Yellow Pine Struts.

Computed from the Formula $1000 - 10\frac{l}{d}$. *l = length in inches d = smaller diameter in inches. This is approximately the same with a factor of safety of 4½, as the Formula* $4250 - 43.3\frac{l}{d}$, *which was obtained by plotting all experiments obtainable, on full-sized sticks, made with the Government Testing Machine at Watertown Arsenal and represents the average of all the lowest breaking weights in each set of tests.*

J. H. Stanwood, Mass. Institute of Technology, 1891.

Size	6 Ft.	8 Ft.	10 Ft.	12 Ft.	14 Ft.	16 Ft	18 Ft.	20 Ft.
4×4	13 100	12 200	11 200					
4×6	19 700	18 200	16 800					
4×8	26 200	24 300	22 400					
4×10	32 800	30 400	28 000					
4×12	39 400	36 500	33 600					
6×6	31 800	30 200	28 800	27 400	25 900	24 500		
6×8	42 300	40 300	38 400	36 500	34 600	32 600		
6×10	52 800	50 400	48 000	45 600	43 200	40 800		
6×12	63 400	60 500	57 600	54 700	51 800	49 800		
6×14	74 000	70 600	67 200	63 800	60 500	57 100		
6×16	84 500	80 600	76 800	73 000	69 100	65 300		
8×8	58 200	56 300	54 400	52 500	50 600	48 600	46 700	44 800
8×10	72 800	70 400	68 000	65 600	63 200	60 800	58 400	56 000
8×12	87 400	84 500	81 600	78 700	75 800	73 000	70 100	67 200
8×14	101 900	98 600	95 200	91 800	88 500	85 100	81 800	78 400
8×16	116 500	112 600	108 800	105 000	101 100	97 300	93 400	89 600
8×18	131 000	126 700	122 400	118 100	113 800	109 400	105 100	100 800
10×10	92 800	90 400	88 000	85 600	83 200	80 800	78 400	76 000
10×12	111 400	108 500	105 600	102 700	99 800	97 000	94 100	91 200
10×14	129 900	126 600	123 200	119 800	116 500	113 100	109 800	106 400
10×16	148 500	144 600	140 800	137 000	133 100	129 300	125 400	121 600
10×18	167 000	162 700	158 400	154 100	149 800	145 400	141 100	136 800
12×12	135 400	132 500	129 600	126 700	123 800	121 000	118 100	115 200
12×14	157 900	154 600	151 200	147 800	144 500	141 100	137 800	134 400
12×16	180 500	176 600	172 800	169 000	165 100	161 300	157 400	153 600
12×18	203 000	198 700	194 400	190 100	185 800	181 400	177 100	172 800
14×14	185 900	182 600	179 200	175 800	172 500	169 100	165 800	162 400
14×16	212 500	208 600	204 800	200 900	197 100	193 300	189 400	185 600
14×18	239 000	234 700	230 400	226 100	221 800	217 400	213 100	208 800
16×16	244 500	240 600	236 800	233 000	229 100	225 300	221 400	217 600
16×18	275 000	270 700	266 400	262 100	257 800	253 400	249 100	244 800
18×18	311 000	306 700	302 400	298 100	293 800	289 400	285 100	280 800
	6 Ft.	8 Ft.	10 Ft.	12 Ft.	14 Ft.	16 Ft.	18 Ft.	20 Ft.

KAOLIN IN ASHLAND, MASS.

By FREDERICK S. HOLLIS, S.B.

Received June 11, 1892.

Basin six, which is being built for the increase of the Boston Water Supply, is formed by building a dam across the valley of Indian Brook at a point about two and a half miles west of the Ashland station.

Indian Brook rises just west of Hopkinton Village, at an elevation of about 380 feet, and for the first two miles of its course runs almost north, following the valley. At this point, however, the direction of the valley changes almost abruptly to east and west, and the brook follows it, running 10° to 15° north of east, which direction it follows, with the exception of one detour toward the south, until it joins the Sudbury River.

The hills which form the valley rise to a height of from 400 to 500 feet, and although mostly covered with drift, there are numerous outcrops showing glaciated rock surfaces. The axes of the hills, as well as the contours on either side of the valley where the dam is built, run approximately east and west, while the outlines of most of the hills in the vicinity are irregular, and do not trend in any one direction.

As a guide in selecting a site for the dam, borings were made at a number of places extending across the valley. It was found that there was considerable overlying drift, and that it increased rapidly in thickness further down the valley. At the point selected, the bottom of the valley in which the brook runs has an elevation of 240 feet, and it was decided to establish the flow line at 295 feet; so the dam is about 60 feet high in the deepest part of the valley, and 1,600 feet long at the top.

The center of the dam is a wall or core of solid masonry. The trench for this core ran almost exactly north and south, and throughout its entire length the earth was removed down to the surface of

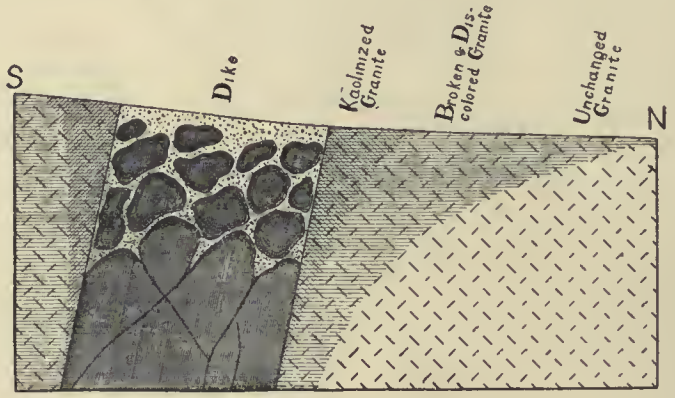
the underlying rock, which is the rather coarse pink granite so common in that locality. At a number of points along this exposure, seams of soft material were found, and through the kindness of Mr. Desmond FitzGerald, engineer and superintendent of the Boston Water Works, I was enabled to examine these on July 27, 1891.

It was evident at first sight that the seams were of two kinds, both as to their composition and their formation. The rock surface of the northern or lee side of the valley slopes rapidly down toward the center, and is very irregular, forming at the upper part of the slope a series of holes, three of which are about ten feet deep. Within these holes, well up on the northern slope, and in a single instance near the beginning of the southern slope, are seams of mica schist. These vary in thickness from those at the top, which are but a few inches, to the three at the bottom, which are from three to five feet. They run northeast and southwest, following in a general direction the joint seams of the rock, and are inclined at an angle of from 20° to 30° from the vertical.

All of these seams are composed of the same dark-colored, fine-grained mica schist; but a large part of the exposed portion has been decomposed, yielding a dark gray product having the same texture as the original rock, but crumbling readily in the fingers. In a few cases the granite which is in direct contact with these seams of schist seemed to be finer and granular. This suggested the possibility of change by contact with the schist; but on careful examination I found that the granite changed independently into this form, which, although crumbly in cases, is probably as firm as when first formed. The only change in the granite that can be attributed to the seams of mica schist seems to be a reddening of the surface. Further down the valley on an exposed rock surface is a seam of schist having the same direction as those exposed in the trench, showing that they are of general occurrence.

The southern or stoss slope, which was more exposed to erosion during the passage of the glacier across the valley, is more gradual, and the surface of the rock has none of the jagged appearance noticed on the northern side. None of the seams of mica schist were observed on this slope, but near the southern end of the dam is a layer of dark-colored rock entirely different from the schist. This is a dike of true eruptive rock, probably diabase from four to five feet wide, running about north 60° east. As exposed in the trench it was much decom-

posed, yielding a loose, black gravel. The decomposition has taken place in the way that is so characteristic of basic dike rocks. The dike rock was first jointed in different directions, yielding roughly cubical blocks; and the decay went on along these joint seams, the corners of the blocks yielding more readily than the flat surfaces, so as to produce rounded stones separated from each other by loose material — the product of their own decay. On breaking a number of these rounded stones the nuclei were found to be of the unchanged dike rock, which was black, rather fine grained and very hard.



The granite intersected by this dike is much cracked for a considerable distance, which could be seen from the material thrown out in digging the trench; and along the wall of the dike the granite is entirely decomposed or kaolinized. This decomposed granite has retained its original form; so that on breaking a piece the quartz, feldspar and hornblende crystals were found to have the same position as in the original rock, and the shape of the feldspar crystals could readily be distinguished, although they are entirely changed to kaolin or white clay.

The change from the granite that is merely cracked to that in contact with the dike, which is entirely decomposed, is gradual and can be easily traced, showing that the granite decayed *in situ*, and that the kaolin is a remnant — probably the lower part of a true sedentary soil. The cracked condition of the granite may be accounted for by the flow of the melted material of the dike up through a fissure in the already formed granite; and the more perfect decomposition along the line of

contact by the fact that the material of the dike, owing to its basic composition, would be decomposed more readily than the granite, thus forming a channel through which water could flow and come in contact with the already cracked or highly jointed granite.

In order to determine the purity of the kaolin, chemical analyses were made; but, on account of the great difficulty in separating a sample, the results are less satisfactory than I hoped they might be. The kaolinized portion is not the result of the decay of a deposit of pure feldspar, nor even of a rock containing large separate crystals of that material, but of the pink Milford granite, containing less than 50 per cent. of feldspar, and that in small, irregular crystals intermixed with and often entirely inclosed by quartz. In taking the first sample for analysis the pieces of kaolinized granite, which were sufficiently moist when collected to form a plastic mass, were allowed to dry, and from freshly broken surfaces the clearer pieces of kaolin were picked out with a sharp pointed knife. This was rubbed between the thumb and finger to separate it from adhering quartz, sifted through a 60-mesh sieve, and dried at 100°C.

The results of analysis are as follows, and for comparison the percentage composition of both pure kaolin and unaltered orthoclase are also given:

	Ashland sample.	Pure Kaolin. Al_2O_3, $2SiO_2$, $2H_2O$.	Orthoclase. $K_2AlSi_6O_{16}$.
Combined water . . .	2.80	13.9	0
SiO_2	82.10	46.3	64.7
Al_2O_3	9.30	39.8	18.4
K_2O	3.90	None, or trace.	16.9
Na_2O	.70	None, or trace.	None, or trace.
CaO	.50	None, or trace.	None, or trace.
MgO	.13	None, or trace.	None, or trace.
Fe_2O_3 and Mn_3O_4 . . .	.65	None, or trace.	None, or trace.

The presence in the Ashland sample of $\frac{2}{9}$ of the amount of K_2O found in unchanged orthoclase, shows the presence of some imperfectly decomposed feldspar. The very large excess of silica and

the correspondingly small amount of combined water are clearly due to the presence of fine quartz which came through the No. 60 sieve in sampling.

In order to get a sample free, if possible, from the admixture of quartz, the clayey portion was separated from a considerable portion of the decomposed granite by levigation with water. This was done by thoroughly mixing a sample with distilled water in a jar about two feet high, and, after allowing to settle for from three to four hours, siphoning off the water with what clay remained in suspension. This was evaporated, and the kaolin thus collected dried at 100°C.

COMPOSITION OF KAOLIN SEPARATED BY LEVIGATION.

Combined water	4.9 per cent.
SiO_2	69.9 "
Al_2O_3	20.6 "
Fe_2O_3	1.3 "
Mn_3O_4	0.1 "

Although the analysis of the sample obtained by levigation gives about twice as much combined water and alumina as the sample obtained by sifting through a 60-mesh sieve, the large excess of silica and the results in combined water and alumina, which are still low, show that there is an admixture of a large amount of fine quartz flour. The amount of combined water in the sample obtained by levigation is 4.9 per cent., or 41 per cent. of what it is in a true clay. For comparison I determined the amount of combined water in a sample of brick clay from the Boston basin, which is a clay owing its origin in a great part to glacial action. It contains 3.4 per cent. of combined water, or only 28 per cent. of the theoretical amount in a true clay.

As to the time when this decomposition of the granite took place, all the evidence seems to indicate that it was mainly if not all preglacial; for the splintered granite alongside of the decomposed portion, and at the same level, has been exposed to the same agencies since glaciation without marked decomposition. At this point the rock is covered with nearly 30 feet of glacial till, which is very compact, forming a hard pan; and throughout this layer are numerous angular fragments of the same granite, broken from neighboring ledges, which have been exposed to the action of infiltrating waters since they were deposited along with the till by glacial action, and their only change is

a discoloration extending but a short distance beyond the surface, caused by the more perfect oxidation of the iron.

The position of the valley favors such a theory; for, as the valley runs east and west, is narrow and has a steep slope toward the north, it is probable that it would soon be filled up with glacial till, over which the ice would pass without coming in contact with or disturbing the surface except along the ridges.

It is evident that all the post-glacial decay must have been accomplished by means of infiltrating water which has filtered through the 30 feet of overlying till; and this must be very slight, for it is situated well up on the southern slope, about 850 feet from the bed of the brook and considerably above it, and well above the peat-covered area. My analyses of the soil, from a position in the valley similar to that above the kaolinized rock, show that there is but little organic matter of any kind except in the superficial covering of loam, which is only from six to nine inches deep.

AMOUNT OF ORGANIC MATTER IN SOIL AT DIFFERENT DEPTHS FROM THE VALLEY OF INDIAN BROOK.

	Loss on ignition.	Per cent. carbon.	Amount of organic matter calculated.
First 6 inches, loam	7.62 per cent.	1.56 per cent.	3.12 per cent.
6 inches to 1 foot, subsoil . . .	4.47 "	.52 "	1.04 "
1½ feet, subsoil	3.48 "	.41 "	.82 "
2½ feet, subsoil	2.48 "	.22 "	.44 "
3 to 3½ feet, subsoil	1.77 "	.13 "	.26 "
3½ to 4 feet, clear sand	.79 "	.03 "	.06 "

THE INSULATION RESISTANCE OF RAIL CIRCUITS IN RAILROAD SIGNALING.[1]

By BERTRAM H. MANN, S.B.

Received July 28, 1892.

For the purposes of railroad signaling by certain systems, the rails of the track are made use of as electric conductors by dividing the track into sections of varying length, according to local circumstances, and placing at one end of each section a battery having its poles connected one to each rail, and at the other end of each section a relay connected in like manner, one pole to each rail. Such a signal is set to "danger," either by shunting the circuit across the rails by the wheels and axles of a train, or by opening the circuit, as when a switch is thrown off the main line, or a rail is broken. The rails, on account of their contact with the ties and with the ground, allow a considerable leakage of the electric current from one rail to the other without passing through the relay, and this portion of the current has no useful effect. The amount of this leakage depends on the *insulation resistance*, and varies with the state of the weather and the condition of the track, etc.; but the insulation resistance of a section of railroad track is always comparatively low. A circuit of normal length, which has on a dry day an insulation resistance of say 5 ohms, may have on a wet day a resistance of not more than $2\frac{1}{2}$ ohms, a difference of 50 per cent. The leakage from one rail to the other is a shunt on the circuit through the relay. Suppose the resistance of the relay to be 9 ohms (about an average value), and the resistance of the line of rails to be 1.2 ohms, and the insulation resistance to be 5 ohms, then the whole resistance external to the battery is $\frac{9 \times 5}{9 + 5} + 1.2$, or 4.4 ohms; on a wet day the same circuit may have an insulation resistance of only $2\frac{1}{2}$ ohms; the total resistance will then be $\frac{9 \times 2.5}{9 + 2.5} + 1.2$, or 3.2 ohms. This change in the insulation resistance will materially alter the amount of current flowing through the relay, as this depends on the difference of potential or pressure between the terminals of the coils; and as this difference of pressure depends on the electro-motive force of the battery, and on the ratio between the resistance of the battery and that of the external

[1] Abstract of thesis in Electrical Engineering.

circuit, it becomes a matter of importance that this should be as uniform as possible, since a uniform current through the coils of the relay will give a constant pull on the armature of the magnet. This will also be affected by the relation between the total resistance and that of the relay magnet; hence this last should be properly proportioned to the circuit in which it is to be used.

For the successful working of signals by means of rail circuits, it is necessary that the electro-motive force of the battery employed should be low, in order that the leakage from rail to rail should not be large. Hence only a few cells can be used on one rail circuit. On some circuits it is necessary to use a relay of much less than the usual resistance, and occasionally to connect the cells in parallel, instead of in series, as is commonly done. In order to give the best results it is necessary to deal with each circuit by itself, and adapt the battery and relay to the particular circuit in which they are to be used. Often the relations of the different quantities must be determined by experiment, and cannot be decided beforehand. Where the rails are always wet (as, for example, in a tunnel), and where the leakage is usually excessive if the battery is connected in the ordinary manner, better results will be obtained by connecting the cells in parallel; that is, all the coppers together for a copper pole, and all the zincs together for a zinc pole. On the other hand, if the circuit be a long and dry one, the best effect will be obtained by connecting the cells in series. In very long circuits it is sometimes necessary to divide the rail circuit into two or more sub-circuits, with a battery and relay for each.

Following are some tables of measurements which have been made of the resistances of rail circuits. The resistance has been measured both in dry and wet weather in some cases. To get the variation, some circuits have been measured when short-circuited by trains in the section, and also with the relay open. The latter gives the resistance of the rails plus that of the insulation. The resistance of these circuits was also taken when they were all clear. This gives the resistance of the rails plus that of the relay as shunted by the leak. The resistance of the rails themselves has been found to vary from about eight tenths of an ohm, in short overlap blocks, to one and six tenths of an ohm on long sections containing switches. This includes the resistance of the underground wiring at the battery end of the section. The insulation resistance of the blocks has varied from two and five tenths ohms, to one hundred and forty ohms.

TABLE NO. I.

BOSTON AND ALBANY RAILROAD. BOSTON TO RIVERSIDE.

RELAYS 9 OHMS.

Date. 1890.	Signal Number.	Cells of Battery in Series.		Rail Lengths in Block.		Length in Feet.		Resistance of circuit external to battery in normal condition.		Weather, and state of road-bed.
		Long Side.	Short Side.	Long Side.	Short Side.	Long Side.	Short Side.	Long Side.	Short Side.	
Feb. 26.	11	2	.....	.....	.....	.....	.....	4.40	.....	Fed through tracks No. 1 and No. 3, into single track at Huntington Ave.
"	12	.....	.....	.....	24 5	.....	735	.....	8.26	Not stated.
"	13	.....	.....	.....	32	.....	960	.....	5.48	" "
"	14	.....	.....	.....	23	.....	690	.....	10.68	" "
"	15	.....	.....	.....	34	.....	1,020	.....	6.45	" "
"	16	.....	.....	84	.....	2,520	.....	5.06	.....	" "
"	18	.....	.....	88	.....	2,640	.....	3.59	.....	" "
Mar. 17.	21	.....	.....	74	.....	2,220	.....	5.65	.....	Comparatively dry.
"	22	.....	.....	.....	35	.....	1,050	.....	6.95	" "
"	23	.....	.....	74	.....	2,220	.....	6.95	"	" "
"	24	.....	.....	.....	34	.....	1,020	.....	7.42	" "
"	25*	.....	.....	.....	.....	.....	.....	.....	.....	
"	27	.....	.....	.....	28	.....	840	.....	7.85	" "
"	28	.....	.....	83	.....	2,490	.....	6.80	.....	" "
"	30	.....	.....	146	.....	4,380	.....	6.45	.....	" "
"	31	.....	.....	108	.....	3,240	.....	4.55	.....	" "
Mar. 15.	"	.....	.....	"	.....	"	.....	3.35	.....	Rails wet; melting snow.
Apr. 12.	"	.....	.....	"	.....	"	.....	3.55	.....	Track dry; 4-ohm relay.
Mar. 15.	33	.....	.. ..	"	.....	"	.....	3.53	.....	Melting snow; quite wet.
"	37	.....	.....	.....	40	.....	1,200	.....	6.75	" " "
Mar. 26.	51	2	.....	161	.....	4,830	.....	4.65	.....	Fair day; ground damp from rains.
"	53	2	.....	"	.....	"	.....	2.75	.....	" " "
"	54	.....	1	.....	42	.....	1,260	.....	5.25	" " "

* No measurements recorded in Mr. Mann's notes.

TABLE NO. 1.—*Continued.*

Date. 1890.	Signal Number.	Cells of Battery in Series.		Rail Lengths in Block.		Length in Feet.		Resistance of circuit external to battery in normal condition.		Weather, and state of road-bed.
		Long Side.	Short Side.	Long Side.	Short Side.	Loog Side.	Short Side.	Long Side.	Short Side.	
Mar. 21.	55	2	.....	49	.....	1,470	.....	5.85	.....	Misty and showery; track wet.
Mar. 26.	"	2	1	.....	44	.....	1,320	.....	6.65	Fair day; ground damp from rains.
"	56	.....	1	.....	41	.....	1,230	.....	5.35	" " "
"	57	.....	1	.....	44	.....	1,320	.....	5.25	" " "
Mar. 21.	"	2	.....	108	.....	3,240	.....	4.53	.....	Misty and showery; track wet.
Mar. 26.	58	2	1	141	45	4,230	1,350	4.65	6.45	Fair day; ground damp from rains.
Mar. 21.	59	2	1	59	44	1,770	1,320	6.35	5.05	Misty and showery; ground wet.
"	60	.....	1	.....	46	.....	1,380	.....	5.7	" " "
Mar. 26.	"	2	.....	141	.....	4,230	.....	3.75	.....	Fair day; ground damp from rains.
Mar. 21.	61	.....	1	.....	34	.....	1,020	.....	5.3	Misty and showery; track wet.
Mar. 19.	"	2	.....	54	.....	1,620	.....	8.45	.....	Heavy snow storm; cold.
Mar. 21.	62	.....	2	.....	47	.....	1,410	.....	10.8	Misty and showery; ground wet.
Mar. 26.	"	2	.....	58	.....	1,740	.....	5.45	.....	Fair day; ground damp from rains.
Mar. 21.	63	.....	1	.....	36	.....	1,080	.....	6.0	Misty and showery; track wet.
Mar. 19.	"	2	.....	53	.....	1,590	.....	7.65	.....	Heavy snow storm; cold.
"	64	.....	1	.....	41	.....	1,230	.....	10.75	" " "
Mar. 21.	"	2	.....	48	.....	1,440	.....	9.55	.....	Misty and showery; track wet.
"	66	3	.....	161	.....	4,830	.....	3.05	.....	" " "
Mar. 19.	"	.....	1	.....	34	.....	1,020	.....	8.15	Heavy snow storm; cold.
"	67	.....	1	.....	36	.....	1,080	.....	8.25	" " "
Total.	.....	.....	.....	.....	.....	.....	.....	120.56	148.79	
Average.	.....	.....	.....	.....	.....	.....	.....	4.82	6.47	

TABLE NO. II.

PROVIDENCE DIVISION OLD COLONY RAILROAD, BETWEEN BOSTON AND HYDE PARK.

RELAYS 9 OR 10 OHMS.

Date. 1890.	Signal Number.	Cells of Battery in Series.		Rail Lengths in Block.		Length in Feet.		Resistance of circuit external to battery in normal condition when circuit was clear.		Weather, and state of road-bed.
		Long Side.	Short Side.	Long Side.	Short Side.	Long Side.	Short Side.	Long Side.	Short Side.	
Apr. 23.	303	2	.....	133	.....	3,990	.....	1.95	.....	Dry and dusty.
"	304	.....	2	.....	60	.....	1,800	.....	4.8	" " "
"	305	2	.....	.....	.....	.....	.....	3.75	.....	" " "
May 7.	305	.....	2	.....	.....	.....	.....	.....	3.8	Fair day; track damp from rain yesterday.
"	303	.....	.....	.....	.....	.....	.....	1.6	.....	" " "
Apr. 23.	308	.....	2	.....	.....	.....	.....	.....	3.65	Dry and dusty.
"	309	2	.....	.....	.....	.....	.....	3.75	.....	" " "
"	312	2	2	.....	.....	.....	.....	4.03	5.35	" " "
"	313	.....	2	.....	.....	.....	.....	.....	6.15	" " "
"	314	2	.....	.....	.....	.....	.....	4.75	.....	" " "
May 7.	337	2	.....	150	.....	4,500	.....	2.34	.....	Fair day; track damp from rain yesterday.
"	341	.....	2	.....	41	.....	1,230	.....	5.30	" " "
Total.	.....	.....	.....	.....	.....	.....	.....	22.17	29.05	
Average.	.....	.....	.....	.....	.....	.....	.....	3.17	4.84	

TABLE NO. III.

NEW YORK AND NEW ENGLAND RAILROAD BETWEEN BOSTON AND DUDLEY STREET.

RELAYS 9 AND 16 OHMS.

Date. 1890.	Signal Number.	Cells of Battery in Series.		Rail Lengths in Block.		Length in Feet.		Resistance of circuit external to battery in normal condition when circuit was clear.		Weather, and state of road-bed.
		Long Side.	Short Side.	Long Side.	Short Side.	Long Side.	Short Side.	Long Side.	Short Side.	
Apr. 30.	1	.....	.....	103	.....	3,090	.....	3.20	.....	Dry.
"	2	.....	.....	.....	.....	.....	.....	4.30	.....	Dry.
"	3	.....	.....	.....	.....	.....	.....	Varied from 4.8 to 3.3	Varied from 5.7 to 5.0	Dry.
"	4	.....	.....	.....	.....	.....	.....	.....	3.73	A 10-ohm and a 16-ohm relay shunted on leak.
"	5	.....	.....	.....	.....	.....	.....	.....	8.54	Dry; one relay on circuit.
"	5	.....	.....	.....	.....	.....	.....	.....	5.76	Dry; two relays shunted on circuit.
"	6	.....	.....	.....	.....	.....	.....	8.43	.....	Dry.

The average of twenty-five measurements on the Boston & Albany Railroad of long sections was 4.82 ohms, and the average of short sections was 6.47 ohms. The average of seven long sections on the Providence Division of the Old Colony Railroad was 3.17 ohms, and of six short sections, 4.84 ohms. In explanation of these low averages it should be said that many of the signals are in yards and other places where there is a considerable accumulation of cinders on the track.

The insulation resistance of any block given in the tables may be found by subtracting the rail resistance from the quantity that is given under the proper "side." The rail resistance varies from eight tenths of an ohm on short sides to one and five tenths ohms on long sides. One and two tenths ohms will be a fair average.

The whole resistance minus the rail resistance will give the resist-

ance of the divided circuit of the relay and insulation. This quantity is equal to the relay resistance multiplied by the insulation resistance and divided by the sum of the two. The relay resistance being known, that of the insulation is readily found. Below will be found the calculation of the insulation resistance of two blocks. The result in both cases is much below the average:

SIGNAL NO. 1, NEW YORK & NEW ENGLAND RAILROAD.

Resistance of the long side with the circuit broken at the relay . . 4.2 ohms.
Resistance of the rails and ground-wires . . . . . 1.6 "
Insulation resistance (the difference of the two) . . . . . 2.6 "
Length of the block . . . . . 3,090 feet.

$$\text{Insulation resistance per mile} = \frac{3090}{5280} \times 2.6 = 1.5 \text{ ohms.}$$

SIGNAL NO. 337, PROVIDENCE DIVISION OLD COLONY RAILROAD.

Resistance of circuit in normal condition . . . . . 2.3 ohms.
Resistance of circuit when short-circuited by a train at the relay end of the section was . . . . . 1.2 ohms.

$2.3 - 1.2 = 1.1$ ohms $= \frac{9S}{9+S}$ where 9 is the resistance of the relay in ohms, and S is the insulation resistance. Solving this equation with respect to S, we have $9.9 = 7.9\,S$; or S, the insulation resistance $= 1.3$ ohms; hence the insulation resistance per mile is equal to $\frac{4500}{5280} \times 1.3$, or 1.1 ohms.

The resistance of the rails is about the same on all circuits when the connections are all in good order.

SIGNAL NO. 303, PROVIDENCE DIVISION OLD COLONY RAILROAD. (Long side.)

Block short-circuited by a train at the relay end . . . 1.1 ohms resistance.
Block short-circuited by a wire at the battery end . . . 0.58 " "

This gives the resistance through 7,980 feet of rails as 0.52 ohm, the difference of the two.

In these measurements there was a path for the current through the wheels and axles of the cars, through the relay, and through the leak. The resistance of the wheels and axles was apparently so small in relation to the resistance of the relay and of the leak in parallel that their effect in lowering the total resistance was neglected. For Signal No. 337 above mentioned, with the long side short-circuited by a train at the relay end, the resistance was 1.2 ohms. The total length of the rails included in the circuit (that is, twice the length of the block)

was 9,000 feet. The measurement (1.2 ohms) gives the resistance of the 9,000 feet of rails, and of the ground wires at the battery end of the section. The rails on this road are connected by two wires around the joints. On the New York & New England Railroad the rails are connected by but one wire around the joints.

Signal No. 1, short-circuited by a train at the relay end, had a resistance of 1.6 ohms.
Short-circuited by a wire at the battery end . . . . 0.6 "

There is then a resistance in the rails themselves of one ohm in about 6,180 feet.

Signal No. 3, short-circuited by a train at the relay end . . . . 1.6 ohms.
Short-circuited by a wire at the battery end . . . 0.6 "

The ground wires in each case were about 60 feet long. The resistances of 1.6 ohms are noted here, as they seem rather high in comparison with others, and especially with 1.2 ohms, as measured on the very long circuit of Signal No. 337, Providence Division Old Colony Railroad, noted above. In the cases of abnormally high resistance it is probable that there is a loose connection or other point of great resistance.

It will be noticed from Table III that the resistance of two signals varied at short intervals of time from (in one case) 5.7 ohms to 5.0 ohms; in the other case, from 4.8 ohms to 3.3 ohms. These variations occurred when trains passed through the block. They might have been caused by the movement of some loose connection during the passage of the train. The resistance was measured in each case with the block clear and the signal at "safety."

There would probably be a considerable increase in steadiness realized by the use of two wires at each joint instead of one. With one exception, where other causes influenced the result, no variation of resistance was noticed when two wires were used. The insulation resistance usually varies from about 2 to about 4 ohms, but in one case it was found to be only 0.53 ohm; but on this circuit there was a large amount of leakage. The average resistance of a long side is less than that of a short side. The contact points of the relays included in the circuit often make a considerable difference in the resistance; in one case removing these from the circuit lowered the resistance by 0.6 of an ohm in a total resistance of about 2 ohms.

On certain circuits it is found an advantage to connect the batteries (of which there are usually two cells on each track circuit) in parallel instead of in series, as is ordinarily done. This gives but half the electro-motive force, it is true, but largely increases the quantity, and in case of the breakage of a cell the current is not totally interrupted, as is the case when they are connected in series.

ROGERS LABORATORY OF PHYSICS,
June, 1891.

NOTE.

Mr. Mann writes me that the apparatus used in these measurements was a portable Wheatstone Bridge, tested in the laboratory of the Massachusetts Institute of Technology and found to be correct to one one-thousandth of an ohm. Accuracy was desired in the measurements only to one one-hundredth of an ohm, on account of the wide variation in measurements of the same piece of track at different times. Several readings of each measurement were, however, taken, and usually found not to vary even in the third place; but the second place only is shown in the records, it being desired that there should be no larger error in the tables as recorded than was probable in the bridge itself. The battery used was that belonging to the track circuit measured, and the bridge was so connected with it that the resistance to be measured formed one branch of the bridge. These measurements are very interesting as being, so far as I know, the first of their kind. I myself once tried to measure the resistance of a line of rails one fourth of a mile in length (in fact one of the very same which Mr. Mann has measured), but found it so small that, with the apparatus I had at command, I obtained no results on which I dared to rely. Such measurements as I did get, however, agreed tolerably well with those obtained by Mr. Mann. I made no attempt to measure the insulation resistance.

GEORGE W. BLODGETT,
Electrical Engineer, Boston & Albany Railroad.

Boston, October 21, 1891.

ON THE PROPERTIES OF HYDROGEN PEROXIDE SOLUTIONS.

By HENRY P. TALBOT, Assistant Professor of Analytical Chemistry, and HERBERT R. MOODY, S.B.

Received August 4, 1892.

The information to be found in the present literature of hydrogen peroxide regarding either the preparation of concentrated solutions of that reagent, or the properties of such solutions after their preparation, is meager and unsatisfactory.

Thenard,[1] the first to identify this chemically interesting substance, concentrated the dilute solutions which he obtained by evaporation *in vacuo* over sulphuric acid. He succeeded in attaining a high degree of concentration, but this method is not suitable for the treatment of large quantities of solution. Since Thenard's work, the most important contribution to this branch of the subject is that made by Hanriot.[2] He found that on partially freezing a solution of hydrogen peroxide the liquid remaining after the removal of the ice contained a larger proportion of peroxide than the original solution, and took advantage of this fact as a means of concentration. In this way he obtained solutions which would yield of themselves 70 to 80 times their own volume of oxygen upon complete decomposition.

Hanriot subsequently found[3] that dilute solutions of hydrogen peroxide could be concentrated by distillation at greatly reduced pressures without serious loss by decomposition; and in this way, under a pressure of 3 mm., he obtained a solution which would evolve 267 times its own volume of oxygen, and states that no appreciable decomposition takes place below 150 volumes.

The writers of the present article have had occasion to deal with commercial solutions, and to prepare from them concentrated solutions

[1] Annales de chimie et de physique [2] 10 (1818), 114, 335; 11 (1819), 85.
[2] Comptes rendus de l'Académie des Sciences, 100 (1885), 57.
[3] Comptes rendus de l'Académie des Sciences, 100 (1885), 172.

in considerable quantities. The facts here presented are those which have been observed during the progress of this work, which have a bearing upon the questions in hand.

The commercial solutions found on the market varied in strength from eighteen volumes to twenty-three volumes, if only those are included which were known to have been freshly prepared. Others were found which were as low as eight volumes.

Kingzett[1] has called attention to the variations in custom as regards the expression of the value of solutions of hydrogen peroxide. He states that a 15-volume solution is properly "one which of itself, by complete decomposition, will furnish fifteen times its own volume of oxygen at ordinary temperature and pressure;" but he points out that the American custom designates as a 15-volume solution one which will evolve fifteen times its own volume of that gas when brought into contact with a substance which itself furnishes one half the volume of oxygen. It is evident that the latter solution has but one half the strength of the former. Since the latter method of expression is that used commercially, it will be employed throughout this article; but attention is called to the fact that for purposes of oxidation the amount of available oxygen is but one half that indicated by the commercial name.

A suggestion made by Dr. F. H. Williams, that the strength of solutions should be uniformly expressed in percentage of hydrogen peroxide, is one which, if generally adopted, would avoid the confusion now existing, and would be more readily understood by physicians.[2]

In view of the fact that hydrogen peroxide is generally recommended as a useful reagent in analytical chemistry, it is desirable that something should be known regarding the purity of the solutions which are readily available. Of the eight commercial samples examined, none were suitable for analytical purposes without purification, and most were so impure that their use would be ill-advised even though blank analyses had been made. They contained hydrochloric, sulphuric, phosphoric and hydrofluosilicic acids, sugar and glycerine. The two latter substances were present in largest amounts in the

[1] Journal of the Society of Chemical Industry, 9 (1890), 4.

[2] It may be noted here, for purposes of comparison, that a 20-volume solution (as the term is used in this article), contains 2.9 per cent. of hydrogen peroxide; a 50-volume solution, 6.9 per cent., and a 100-volume solution, 13.4 per cent., by weight.

sample prepared for medicinal use. Barium, calcium, aluminum and magnesium were also found.

The amount of phosphoric acid, as found by precipitation with ammonium molybdate and reprecipitation as magnesiun ammonium phosphate, was in one sample 0.16 per cent., in another 0.13 per cent. of the solution by weight; an amount capable of doing much mischief in analytical work.

Fifty cubic centimeters of a commercial sample evaporated on a water bath left a residue weighing 0.0588 grm., of which 0.0418 grm. remained after ignition. This sample contained sulphuric, phosphoric and hydrofluosilicic acids and barium. A sample intended for medicinal use yielded a syrupy residue on evaporation, the amount from 50 cc. weighing 0.4400 grm.; at 130° C. it was still syrupy, but had the odor of caramel, and the weight had fallen to 0.4195 grm.; upon ignition over a Bunsen burner glycerine distilled, and the sugar was charred and burned, the residue, after treatment with ammonium nitrate, being white, and weighing 0.0164 grm. This residue was largely barium chloride.

The presence of hydrofluosilicic acid in these solutions is also a menace to analytical work since, upon subsequent neutralization of the acid, the precipitation of an insoluble hydrofluosilicate or of silicic acid takes place, either of which is likely to interfere with gravimetric processes. The amount of hydrofluosilicic acid found in one sample — a 22-volume solution — was 0.05 per cent. of the solution by weight. The acid was precipitated and weighed as potassium silicofluoride, and was also determined from the amount of silica present.

An order placed with one of the manufacturers for a preparation of hydrogen peroxide which should be sufficiently pure for analytical purposes resulted in the production of a solution which was free from phosphoric acid, and yielded but 0.0049 grm. of ignited residue from 50 cc. of the sample. The amount of silica found was 0.0026 per cent., and only minute quantities of calcium and aluminum, with a little organic matter, were present. This sample might have been used for analytical purposes without further purification.

As a method of purification of the commercial solutions, the following was found most successful. The solution was treated with about 10 per cent. by volume of alcohol, after which barium hydrate in powder was added to distinctly alkaline reaction. The precipitate, containing most of the impurities, was then filtered off with the help of a

gentle suction, using a porcelain filter plate, and the excess of barium removed by the addition of sulphuric acid in excess. The barium sulphate was allowed to settle, and the supernatant liquid quickly filtered as before. The loss of peroxide is small if the first filtration is hastened, thereby reducing the time during which the solution remains alkaline. By this method the amount of silica was reduced from 0.028 per cent. to 0.003 per cent. of the solution by weight. The alcohol may be removed by distillation at reduced pressure, and the residual solution may then be used for analytical purposes without further precaution than the customary blank analysis. The barium hydrate used should be as free from carbonate as possible.

It was subsequently found that a solution thus treated could be further purified by distillation, but only at a great sacrifice of material. This will be referred to again.

The addition of ferric chloride and subsequent precipitation with barium hydrate cannot be employed as a means of purification, since the formation of ferric hydrate in the solution causes prompt and complete decomposition of the peroxide which the previous addition of alcohol, ether, or their mixture, does not hinder.

It was found that the concentrated solutions containing phosphoric and hydrofluosilicic acids, when shaken with ether, gave up a portion of these acids together with the peroxide. Purification by this means, therefore, was not possible. Experiments proved that for practical purposes concentration by distillation at reduced pressure is preferable to freezing. The distillations were made in glass — an ordinary round-bottomed flask or a boiling flask, a Liebig condenser and glass receiver comprising the apparatus. The stoppers were of rubber. For distillations at pressures less than that of the atmosphere the apparatus was connected with a suction pump capable of reducing the pressure to 30 or 40 mm. To prevent violent bumping, it was usually necessary to insert a capillary tube and draw a slow current of air through the solution in the boiling flask. This bumping is, in itself, an evidence that the decomposition of the peroxide is slight. To prevent decomposition by over-heating, the flask was placed in a water bath.

The rate at which the solutions concentrate under varying conditions may be learned from the tables below. The column marked "Per cent. total loss" expresses the percentage of hydrogen peroxide which the solution has lost during the distillation, and is calculated upon the theoretical concentration, which, in turn, is estimated from

the decrease in volume of the solution in the boiling flask, and takes no account of the small quantity of peroxide which passes into the distillate. The commercial solution employed was a 20-volume solution, containing as impurities, phosphoric, hydrofluosilicic, sulphuric and hydrochloric acids, and a small quantity of iron.

The rate of concentration of the residual solution in the boiling flask was determined by titration with potassium permanganate. One cubic centimeter was removed with a pipette, diluted with fifty to seventy-five cubic centimeters of distilled water, acidified with sulphuric acid, and titrated to the customary end point. This was the method employed in all determinations mentioned, and a comparison of this method with a gas-volumetric process will be made at the close of the article.

CONCENTRATION BY DISTILLATION.

I. AT ATMOSPHERIC PRESSURE.				II. UNDER PRESSURE OF 390 mm.			
Volume of Distillate.	Strength of Distillate.	Strength of Residual Solution.	Per cent. Total loss.	Volume of Distillate.	Strength of Distillate.	Strength of Residual Solution.	Per cent. Total loss.
65 cc.	0.6 vols.	16.5 vols.	30.6	50 cc.	0.6 vols.	22.9 vols.	0.0
65 cc.	0.6 "	20.9 "	29.3	60 cc.	0.8 "	26.2 "	4.7
65 cc.	0.5 "	21.2 "	45.6	50 cc.	0.6 "	30.2 "	9.3
65 cc.	0.6 "	26.5 "	53.5	50 cc.	0.5 "	38.5 "	8.4
65 cc.	0.8 "	32.5 "	69.5	50 cc.	0.8 "	42.2 "	26.0
50 cc.	2.24 "	81.0 "	70.4	50 cc.	0.8 "	52.8 "	40.5
.....	.....	.....	.....	40 cc.	2.0 "	73.1 "	54.3
.....	.....	.....	.....	20 cc.	1.1 "	84.0 "	68.4

Original volume = 400 cc.
Original strength = 20 vols.

CONCENTRATION BY DISTILLATION.

III. UNDER PRESSURE OF 140 mm.				IV. UNDER PRESSURE OF 30 mm.			
Volume of Distillate.	Strength of Distillate.	Strength of Residual Solution.	Per cent. Total loss.	Volume of Distillate.	Strength of Distillate.	Strength of Residual Solution.	Per cent. Total loss.
55 cc.	0.5 vols.	23.1 vols.	0.0	50 cc.	0.9 vols.	22.0 vols.	2.7
55 cc.	0.8 "	26.8 "	0.8	50 cc.	0.9 "	25.2 "	5.0
55 cc.	0.8 "	30.8 "	9.4	50 cc.	0 9 "	29.6 "	6.9
55 cc.	0.8 "	39 0 "	12.1	50 cc.	0.9 "	37.8 "	5.5
65 cc.	1.2 "	63.6 "	8.4	50 cc.	0.9 "	49.8 "	7.7
60 cc.	2.0 "	138.7 "	4.6	50 cc.	1.3 "	77.2 "	2.7
20 cc.	6.3 "	189.4 "	7.0	50 cc.	9.4 "	151.2 "	4.8

Original volume = 400 cc.
Original strength = 20 vols.

Original volume = 400 cc.
Original strength = 19.84 vols.

CONCENTRATION BY DISTILLATION.

V. AT ATMOSPHERIC PRESSURE WITH THE ADDITION OF PHOSPHORIC ACID.				VI. AT 390 mm. PRESSURE WITH THE ADDITION OF PHOSPHORIC ACID.			
Volume of Distillate.	Strength of Distillate.	Strength of Residual Solution.	Per cent. Total loss.	Volume of Distillate.	Strength of Distillate.	Strength of Residual Solution.	Per cent Total loss.
60 cc.	0.6 vols.	21.1 vols.	10.8	40 cc.	0.6 vols.	21.5 vols.	4.0
60 cc.	0.7 "	24.5 "	15.0	50 cc.	0.6 "	22.3 "	13.9
60 cc.	0.8 "	27.9 "	23.5	50 cc.	0.9 "	26 5 "	14.2
60 cc.	0.8 "	35.6 "	29.2	50 cc.	1.3 "	31.5 "	18.0
65 cc.	1.9 "	54.2 "	35.9	50 cc.	0.9 "	37.8 "	24.7
60 cc.	3.1 "	69.0 "	70.0	60 cc.	1.9 "	53.9 "	32.9
25 cc.	8.5 "	0.3 "	99.9	50 cc.	4.1 "	107.3 "	33.2
.....	.....	.....	.....	45 cc.	19.2 "	6.3 "	99.8

Original volume = 400 cc.
Original strength = 20.2 vols.

It is obvious that the figures representing the loss by decomposition in these tables must be approximate only, since this is influenced by the amount of mechanical agitation of the solution and the length of time which it has stood at the temperature of the laboratory previous to distillation. Another factor which influenced the results when the distillation was made at very low pressure, was the removal of uncondensed steam through the suction pump, the low temperature at which the solution boils (36° C. for 30 mm. pressure), making it difficult to effect complete condensation. Tables I to IV, inclusive, show approximately the rate of concentration under varying conditions, and demonstrate the fact that strong solutions may be prepared by distillation at a pressure of 30 to 40 mm. without serious loss. The addition of certain non-volatile substances as preservatives during distillation was not attended with a large degree of success. Sulphuric acid, boracic acid, phosphoric acid and glycerine were each tried; but of these phosphoric acid alone appeared to hinder decomposition, and that only within certain limits, as shown by Tables V and VI. At the lower pressure the results are more favorable than in the absence of the acid, until the solution is highly concentrated. Ten cubic centimeters of a saturated solution of phosphoric acid were used in the first case (Table V), and five cubic centimeters in the second. Of the action of the remaining acids and glycerine it may be said that they appear to hinder decomposition only at the first, after which they increase rather than lessen the loss. It may be mentioned that the presence of an excess of caustic alkali or ammonia causes prompt and complete decomposition of the peroxide.

The figures given in Tables III and IV show that appreciable quantities of hydrogen peroxide pass into the distillates as the solution in the boiling flask concentrates. This fact may be utilized as a method of purification of the peroxide solutions from most of the foreign ingredients. It is not wholly successful, since the hydrofluosilicic acid is present in such large quantities that, at the point at which the peroxide begins to pass over in any considerable quantity, this acid is decomposed and silicon fluoride passes into the distillate with the re-formation of hydrofluosilicic acid. This was made evident by the accumulation of silicic acid in the condenser from the partial decomposition of the silicon fluoride by condensed steam. The distillate is free from other impurities than the hydrofluosilicic acid, and by a second concentration a very pure solution could doubtless be obtained, but at a great sacrifice of material. By subjecting the fil-

trate from the precipitation with barium hydrate in the presence of alcohol, as already described, to distillation at reduced pressure, it is easy to obtain a distillate which, while weak, contains but a minute quantity of hydrofluosilicic acid. It may be concentrated after the addition of such a quantity of sulphuric acid that, when concentrated to a 20-volume solution, it shall contain about 0.2 per cent. by weight of the acid.

The statement is made by Hanriot[1] that pure hydrogen peroxide gives an acid reaction; but distillates prepared as just described, capable of evolving nine volumes of oxygen, are but very feebly acid (which may be accounted for by the presence of hydrofluosilicic acid and hydrofluoric acid in very small quantities), and when made neutral by a drop of an alkali solution remain so, even though the amount of peroxide is undiminished. It appears to us that the statement of Hanriot cannot be correct, although he claims to have proved the absence of acid in the solutions tested.

The data already presented make it evident that the preparation of concentrated solutions of hydrogen peroxide from the commercial solutions is a matter of but little difficulty, unless the complete absence of foreign matter is required. The writers have prepared considerable quantities of solutions, varying in strength from thirty to five hundred and eighty-eight volumes. It has generally been found expedient to concentrate the solution to about fifty volumes before making any attempt to purify, unless the purification is accomplished by precipitation with barium hydrate, in which case the precipitation must precede the concentration. A convenient procedure by which the impurities may be largely removed is the following: The solution is made neutral with caustic potash (as free from carbonate as possible), and the precipitate of silica, potassium silicofluoride and other impurities is allowed to settle. This takes place promptly. The supernatant liquid is then decanted through a ribbed filter, and the last portion, which contains the precipitate, and therefore filters slowly, is rejected, or preserved for re-concentration if necessary. In this way a solution is obtained which is free from most of the original impurities, but contains potassium salts of the acids present. The filtered liquid should be made acid with sulphuric acid as soon as filtered, to prevent the decomposition of the peroxide in neutral solution, and the

[1] Comptes rendus de l'Académie des Sciences, 100 (1885), 172.

amount of acid added to a 50-volume solution should be about 0.5 per cent. by weight. An equivalent amount of hydrochloric acid may be substituted for the sulphuric acid.

In the preparation of solutions above fifty or sixty volumes it is expedient to purify with caustic potash, if this is the method used, before concentrating beyond this point, since the stronger solutions decompose with great rapidity as soon as the acid is neutralized.

The loss of strength is very gradual even in highly concentrated solutions, provided there is free acid present. The accompanying table will furnish data in support of this statement.

STABILITY OF CONCENTRATED SOLUTIONS.

No.	Nature of Solution.	Acidity in terms of H_2SO_4.	Original Strength.	Time of Standing.	Temperature.	Final Strength.	Per cent. loss.
1.	Commercial solution.	Approx. 0.2 per cent.	19.4 vols.	3 days.	70° to 80° F.	19.4 vols.	0 0
		.....	.....	7 "	" "	19.2 "	1.0
		.....	.....	13 "	" "	19.2 "	1.0
2.	Commercial solution concentrated at reduced pressure.	Approx. 0.5 per cent.	44.6 vols.	3 days.	70° to 80° F.	44.6 vols.	0.0
		.....	.....	7 "	" "	43.5 "	2.4
		.....	.....	13 "	" "	43.0 "	3.6
		.....	.....	42 "	" "	40.4 "	9.4
		.....	.....	128 "	" "	27.9 "	37.4
3.	Commercial solution concentrated and purified by KOH.	Sufficiently acid to redden litmus.	41 vols.	2 days.	70° to 80° F.	41.2 vols.	0.0
		.....	.....	29 "	" "	32.0 "	22.3
		.....	.....	115 "	" "	11.9 "	71.4
4.	Commercial solution concentrated and purified by KOH.	Nearly neutral.	47.5 vols.	2 days.	70° to 80° F.	42.1 vols.	11.3
		.....	.....	22 "	" "	7.0 "	85.2
		.....	.....	110 "	" "	0.5 "	98.9
5.	Commercial solution concentrated at reduced pressure.	0.9 per cent.	99.4 vols.	3 days.	70° to 80° F.	100 3 vols. (?)	0.0
		.....	.....	17 "	" "	82.7 "	16.8
		.....	.....	24 "	" "	76.0 "	23.4
6.	Commercial solution concentrated at reduced pressure.	1.9 per cent.	202 vols.	3 days.	70° to 80° F.	188.0 vols.	6.9
		.....	.....	14 "	" "	130.0 "	35 6
		.....	.....	21 "	" "	105.0 "	48.0
		.....	.....	86 "	" "	22 8 "	88.2
7.	Commercial solution concentrated.	Alkaline with KOH.	34.4 vols.	2 days.	70° to 80° F.	9.10 vols.	73 5
		.....	.....	8 "	" "	1.8 "	94 8
8.	Commercial solution concentrated.	Alkaline with NH_4OH.	28.9 vols.	2 days.	70° to 80° F.	0.6 vols.	97.9
		.....	.....	8 "	" "	0.0 "	100.0

STABILITY OF CONCENTRATED SOLUTIONS.

No.	Nature of Solution.	Acidity in terms of H_2SO_4.	Original Strength.	Time of Standing.	Temperature.	Final Strength.	Per cent. loss.
9.	Commercial solution concentrated at reduced pressure.	3.7 per cent.	390 vols.	13 days.	32° F.	336 vols.	13.8
		.....	.....	19 "	"	332 "	14.8
		.....	.....	30 "	"	310 "	20.5
		.....	.....	37 "	"	290 "	25.6
10.	Commercial solution concentrated at reduced pressure.	2.9 per cent.	300 vols.	9 days.	32° F.	274.0 vols.	8.6
		.....	.....	15 "	"	271.4 "	9.5
		.....	.....	26 "	"	250.8 "	16.4
		.....	.....	33 "	"	240 0 "	20.0
11.	Prepared from No. 10 by dilution.	1.9 per cent.	202 vols.	3 days.	32° F.	201.6 vols.	0.1
		.....	.....	6 "	"	194.0 "	3.9
		.....	.....	17 "	"	180 6 "	10 0
		.....	.....	24 "	"	169.7 "	16.0
12.	Prepared from No. 10 by dilution.	0.9 per cent.	100 vols.	3 days.	32° F.	99.4 vols.	0.6
		.....	.....	6 "	"	98.6 "	1.4
		.....	.....	17 "	"	96.1 "	3.9
		.....	.....	24 "	"	95.0 "	5.0

A comparison of the solutions numbered 2 to 8, inclusive, shows that those which have been prepared by the evaporation of commercial solutions, without neutralization, maintain their strength for a considerable length of time; while those which have been made neutral decompose in a few days, and an excess of alkali causes immediate deterioration. In some cases the decomposition upon the addition of the alkali was spontaneous and accompanied by a marked evolution of heat. Solutions which have been even partially neutralized by alkali carbonates deteriorate during the neutralization, the carbonic acid evolved acting mechanically to destroy the peroxide.

It is also evident from the foregoing table that solutions of less strength than 100 volumes may be kept for some time at the temperature of the laboratory; but an examination of solutions 9 to 12, inclusive, shows that the highly concentrated solutions are preserved for much longer periods if kept at low temperatures. The stronger solutions — from 50 to 400 volumes — find their chief use in medicine and dentistry, and a loss of 10 per cent. in strength does not interfere with their practical efficiency. A glance at the table shows that, even in the case of the strongest solution, several days elapse before the value of the solution diminishes to that extent. It should be mentioned that these solutions were stoppered, and were only disturbed when the samples were removed for analysis. The deterioration would have been slightly greater if the solutions had been frequently exposed to the air or frequently shaken; but the fact is demonstrated that, if reasonable precautions are taken, even 400-volume solutions, if freshly prepared, may be preserved sufficiently long to admit of their practical use by physicians in extreme cases. They should be carefully stoppered, kept at a low temperature, out of contact with daylight, and should be disturbed as little as possible. To what extent the permanency of the strong solution may be increased by the addition of the preservatives recommended by Kingzett for commercial solutions, the writers have not yet determined.

Finally, a few words regarding the practical value of the permanganate method for the determination of the amount of the hydrogen peroxide present. If, as has been stated, the oxygen evolved during the treatment with permanganate is not furnished equally by that reagent and the hydrogen peroxide, the titration should not yield accurate results. For the purpose of comparison, the amount of oxygen evolved by the solution alone was determined with the aid of the

following apparatus: A boiling flask, of 30 cc. capacity, was connected at its neck, by means of a T tube, with a carbonic acid generator, and with a 2 cc. pipette. The delivery tube from the boiling flask was connected with a Mitscherlich bulb surrounded by ice, and that, in turn, with an azotometer filled with a caustic potash solution (sp. gr. 1.27). The air was first expelled from the apparatus by a current of carbonic acid, after which the stopper of the flask was removed to insert the hydrogen peroxide (2 cc.), and the small quantity of air introduced was again removed. The introduction of the peroxide was delayed until the greater portion of the air had been expelled, to prevent possible decomposition due to the contact with the carbonic acid for a longer time. Finally, 2 cc. of a solution of caustic potash (sp. gr. 1.27) was admitted through the pipette and T tube, which caused immediate evolution of oxygen. A gentle pressure of carbonic acid was maintained, and the flask heated in an oil bath until the solution had completely evaporated. The Mitscherlich bulb acted as a condenser, but no trace of undecomposed peroxide could be found in the condensed liquid. The oxygen passed into the azotometer and was measured, transferred to a Hempel pipette, the oxygen absorbed in potassium pyrogallate and the small amount of air deducted. The residual volume was corrected for temperature and pressure. The results were as follows:

A solution which, from the titration with potassium permanganate, should evolve 10.08 volumes of oxygen at 0° and 760 mm., was found, by the gas-volumetric method just described, to evolve 10.12 and 10.22 volumes (corrected), the variation being 0.4 and 1.4 per cent. respectively; showing that for practical purposes the results obtained by titration with a solution of potassium permanganate, which has been standardized against iron wire, are sufficiently accurate. No further comparisons have yet been made of this gas-volumetric method with other methods; but the apparatus is so easily constructed, and the process is so simple and rapid, that it commends itself as a ready means for the determination of the amount of oxygen which the peroxide solution alone will furnish.

In conclusion, the writers desire to express their indebtedness to Dr. T. M. Drown for valuable suggestions and kind interest throughout the work.

Massachusetts Institute of Technology,
July, 1892.

THE VOLATILITY OF STANNIC CHLORIDE.

By THOMAS M. DROWN and GEORGE F. ELDRIDGE.

Received August 20, 1892.

It is well known that there is a loss of tin in evaporating solutions of stannic chloride; but the amount of tin thus lost, under different conditions of dilution, temperature and acidity, seems not to have been hitherto determined. Owing to our lack of knowledge on this subject we have not dared to evaporate even very dilute solutions of stannic chloride in cases where the amount of tin was to be determined, nor have we known whether tin could be completely volatilized as stannic chloride when associated with other metals. The following investigation was undertaken to ascertain some facts with regard to the volatility of stannic chloride.

It is not an easy matter to determine directly the amount of tin lost on evaporation on account of the difficulty of condensing completely the vapors of stannic chloride. For this reason the amount of tin volatilized was in all cases determined by loss.

As the work planned involved more than a thousand determinations of tin, it was necessary to have an accurate and rapid volumetric method for this purpose. The following method we found left nothing to be desired, both as regards accuracy and rapidity. To a small bulk of tin solution, say 5 cc., is added from two to three tenths of a gram of powdered zinc,[1] which reduces the tin practically instantaneously to the metallic state. Forty cubic centimeters of hydrochloric acid of 1.2 sp. gr. are then added, which dissolves the finely divided metallic tin within a minute. A solution of ferric chloride is now added in slight excess, and the amount of ferrous chloride formed in the reaction with the stannous chloride is determined by titration with potassium bichromate, using potassium ferricyanide as an indicator.

[1] The powdered zinc is made by shaking melted zinc in a closed vessel near the point of solidification. It is then sifted to obtain a powder of uniform grain. That used in these experiments passes through a sieve of sixty meshes to the linear inch.

The following determinations, made by dissolving pure tin in hydrochloric and oxidizing to the stannic condition, show the limits of accuracy of the process:

	Tin taken. Gram.	Tin determined. Gram.
1	0.8542	0.8549
2	0.1724	0.1702
3	0.5638	0.5630
4	0.0922	0.0932
5	0.1749	0.1742
6	0.2348	0.2357
7	0.0124	0.0129
8	0.7721	0.7708
9	0.2432	0.2435
10	0.4651	0.4664

It was found that it made no difference whether the ferric chloride was added to the hot or cold solution of stannous chloride, and also that the solution of the stannous chloride could stand at least five hours (before the addition of the ferric chloride and the titration with potassium bichromate) without noticeable change.

The stannic chloride used in these experiments was made by dissolving pure tin in hydrochloric acid and oxidizing by chlorine gas. The chlorine and the greater part of the excess of hydrochloric acid were driven off by evaporation, and the semi-solid residue of stannic chloride dissolved in water. Ten cubic centimeters of this solution were taken in the various experiments, and diluted to exactly twenty cubic centimeters with water or acid, according to the object of the experiment. This amount of solution was placed in a round-bottomed flask of 200 cc. capacity, which was immersed to its neck in a bath of high-boiling petroleum oil. The amount of tin volatilized at different temperatures and during different times was determined by taking a sample from the stannic chloride remaining in the flask. There was undoubtedly some stannic chloride condensed in the neck which flowed back into the flask, so that the losses as determined

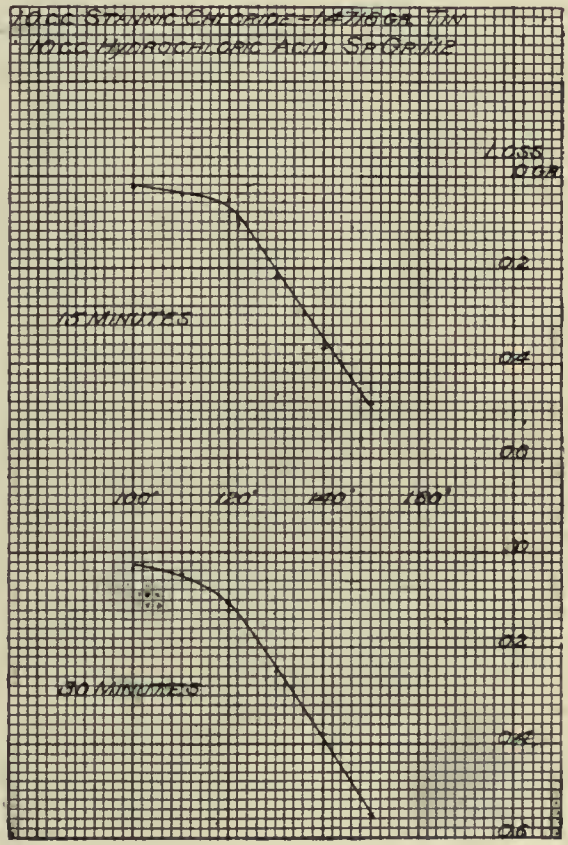
by this method are somewhat lower than would be the case in open dishes, or if the flask had been immersed in the oil bath to the top of the neck.

The evaporations at the different temperatures were all made in duplicate, and four determinations were made of the tin remaining in the flasks in all cases. In the first series of evaporations in flasks a more concentrated solution of stannic chloride was used than in the subsequent experiments. The results of this series are as follows:

10 cc. stannic chloride containing 1.4716 grams tin and 10 cc. hydrochloric acid (sp. gr. 1.12).

Temperature. Centigrade degrees.	Time. Minutes.	Loss of Tin, Gram.
100	15	0.0206
100	30	0.0354
110	15	0.0445
110	30	0.0484
120	15	0.0546
120	30	0.1025
130	15	0.2185
130	30	0.2523
140	15	0.3069
140	30	0.3795
150	15	0.4835
150	30	0.5328

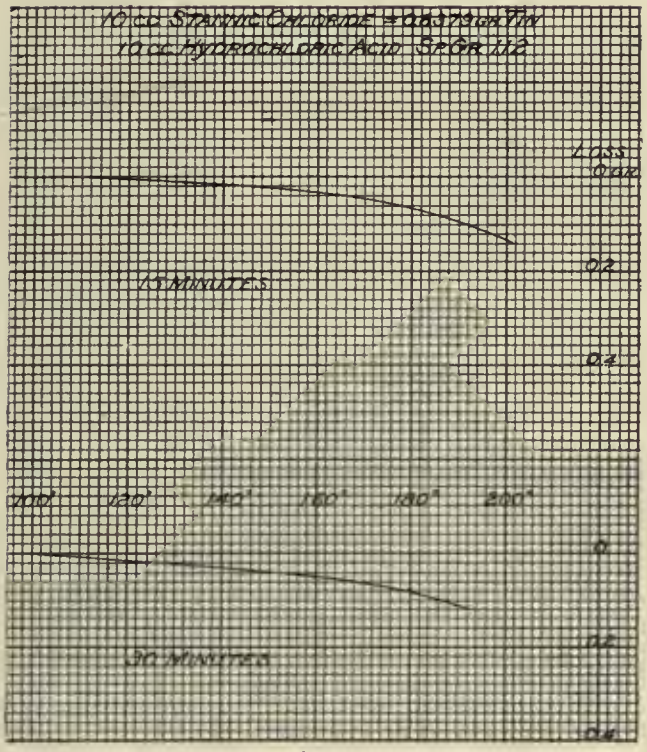
These results show that an acid, concentrated solution of stannic chloride cannot be heated fifteen minutes to 100° without the loss of an appreciable amount of tin, and that at higher temperatures the loss is very rapid. The results of this series are shown graphically in the following diagram :



A solution of stannic chloride containing less than half the amount of tin used in the previous experiment was next tried under the same conditions. The results were as follows:

10 cc. stannic chloride containing 0.6379 gram tin and 10 cc. hydrochloric acid (sp. gr. 1.12).

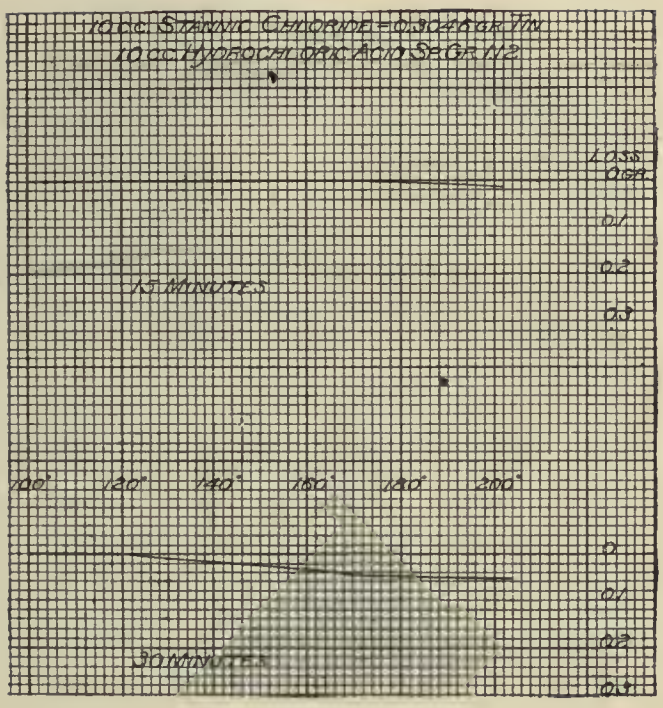
Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
100	15	None.
100	30	None.
110	15	None.
110	30	0.0084
120	15	0.0042
120	30	0.0162
130	15	0.0108
130	30	0.0240
140	15	0.0180
140	30	0.0310
150	15	0.0222
150	30	0.0389
160	15	0.0346
160	30	0.0506
170	15	0.0465
170	30	0.0638
180	15	0.0619
180	30	0.0750
190	15	0.0935
190	30	0.1143
200	15	0.1310
200	30	Decomposed.



These results point to the fact that a certain concentration must be reached before any tin is volatilized from its solutions. The greater volatility of the more concentrated solution is well shown in comparing the two diagrams. A still more dilute solution of tin was used in the next experiment.

10 cc. stannic chloride containing 0.3046 gram tin and 10 cc. hydrochloric acid (sp. gr. 1.12).

Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
100	15	None.
100	30	None.
110	15	None.
110	30	None.
120	15	None.
120	30	None.
130	15	None.
130	30	0.0125
140	15	None.
140	30	0.0167
150	15	None.
150	30	0.0232
160	15	None.
160	30	0.0333
170	15	None.
170	30	0.0475
180	15	0.0041
180	30	0.0493
190	15	0.0071
190	30	0.0482
200	15	0.0131
200	30	0.0487

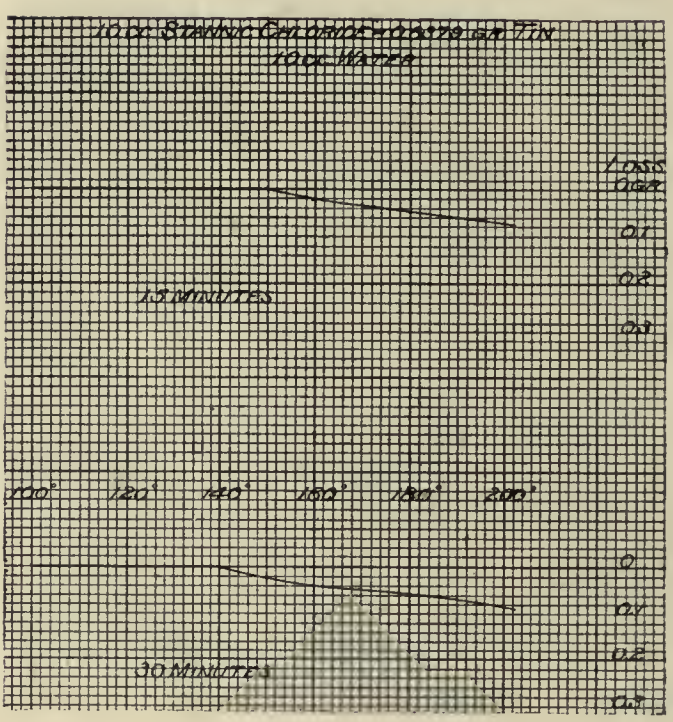


These results confirm those of the previous series, that dilute solutions of stannic chloride may be heated for some time without loss of tin.

To determine the effect of the amount of free acid on the volatility of the tin a solution was prepared, in which the 10 cc. of acid were replaced by 10 cc. of water.

10 cc. stannic chloride containing 0.6379 gram tin and 10 cc. water.

Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
150	15	None.
150	30	0.0232
160	15	0.0268
160	30	0.0398
170	15	0.0298
170	30	0.0470
180	15	0.0516
180	30	0.0584
190	15	0.0595
190	30	0.0681
200	15	0.0748
200	30	0.0840

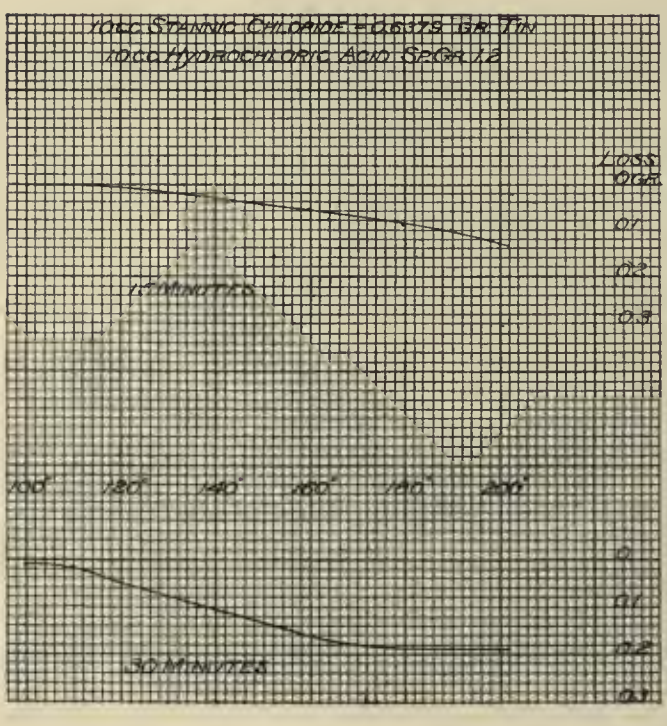


This series is comparable with the second series in amount of tin, and it will be noticed that whereas in this series no loss of tin occurs until the solution is heated to 150° for thirty minutes, in the series containing the free hydrochloric acid a notable loss occurred when the solution was heated for thirty minutes to 110°.

In all the experiments there was observed a decomposition of the stannic chloride with the formation of a non-volatile hydrate of tin when a certain concentration was reached. In the case of the acid solution (second series) this decomposition began at 180°; in the last series, in which the acid was replaced by water, the decomposition began at 160°. The presence of free hydrochloric acid is thus shown to be favorable to the volatilization of tin, both at low and at high temperatures. In the next series the same amount of tin was used, but a stronger hydrochloric acid.

10 cc. stannic chloride containing 0.6379 gram tin and 10 cc. hydrochloric acid (sp. gr. 1.2).

Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
100	15	None.
100	30	0.0132
110	15	None.
110	30	0.0150
120	15	0.0071
120	30	0.0489
130	15	0.0191
130	30	0.0941
140	15	0.0335
140	30	0.1065
150	15	0.0489
150	30	0.1136
160	15	0.0554
160	30	0.1607
170	15	0.0739
170	30	0.1798
180	15	0.0881
180	30	0.1875
190	15	0.0995
190	30	0.1864
200	15	0.1346
200	30	0.1893

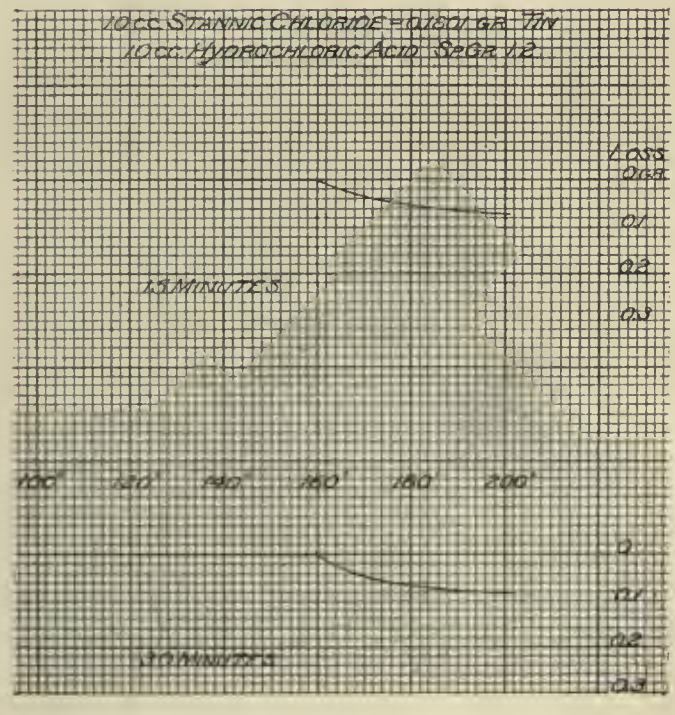


These results confirm those of the previous series in showing the favorable influence of hydrochloric acid on the volatilization of the tin. It will also be noticed in this series that at the three highest temperatures, namely, 180°, 190° and 200°, the loss in thirty minutes is practically the same. At 180° the solution goes to dryness in thirty minutes, and the tin being then in the form of hydrate, no further volatilization is possible. The same result took place with the more dilute solution of tin (as shown in the third series of experiments) at a temperature of 170°.

The next series of experiments was made to determine the effect of strong hydrochloric acid on very dilute solutions of tin. A few cubic centimeters of the solution used in the previous series were diluted with hydrochloric (sp. gr. 1.2) to 10 cc., and 10 cc. additional of hydrochloric acid added.

10 cc. stannic chloride containing 0.1801 gram tin and 10 cc. hydrochloric acid (sp. gr. 1.2).

Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
160	15	None.
160	30	None.
170	15	0.0366
170	30	0.0445
180	15	0.0525
180	30	0.0617
190	15	0.0638
190	30	0.0750
200	15	0.0710
200	30	0.0823



There is thus shown to be no loss of tin in this dilution until a temperature of 170° is reached, notwithstanding the large amount of free acid present. A certain concentration of the stannic chloride is necessary before volatilization can occur.

It has already been mentioned that there is no further loss of tin after the chloride is decomposed and the hydrate formed. An attempt was made to determine the point at which a solution of stannic chloride begins to decompose at different temperatures. A series of flasks containing the same amount of stannic chloride and hydrochloric acid were heated in oil baths at different temperatures, and the time noted when the solutions showed by their turbidity that the hydrate was beginning to form. The amount of tin lost during these periods was then determined.

10 cc. stannic chloride and 10 cc. hydrochloric acid (sp. gr. 1.2).

Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
100	270	0.0814
110	225	0.0766
120	120	0.0840
130	90	0.0768
140	60	0.0786
150	45	0.0846
160	40	0.0780
170	28	0.0734
180	25	0.0886
190	18	0.0847
200	15	0.0814

These results show that under the same conditions of concentration and acidity the stannic chloride begins to decompose when reaching a certain concentration, which is the same for all the temperatures tried. The volume of liquid remaining in the flasks was from 2.3 to 2.5 cc.

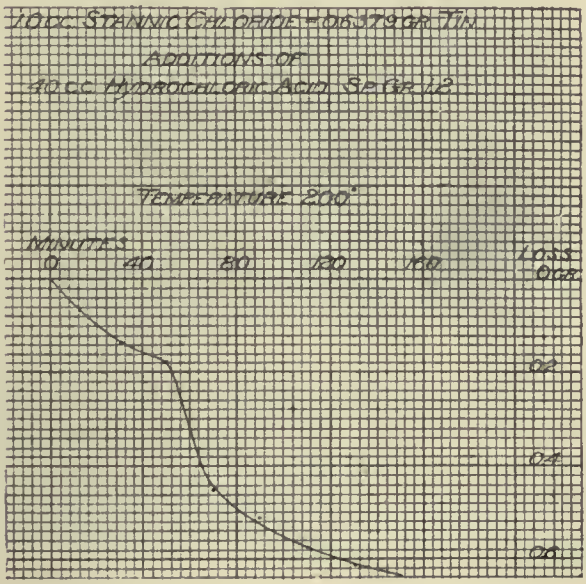
The limit to the loss of tin in all the previous experiments has been the decomposition of the stannic chloride. It seemed reasonable to suppose that by repeated additions of hydrochloric acid the tin

could be completely volatilized. This was shown to be the case. In the following series 10 cc. of the stannic chloride solution were diluted with 40 cc. of strong hydrochloric acid and evaporated to dryness at a temperature of 200°. The residue was dissolved in hydrochloric acid and the loss of tin determined. To other flasks 40 cc. of hydrochloric acid were added after each evaporation to determine the number of evaporations and additions of acid to volatilize the tin completely.

10 cc. stannic chloride containing 0.6379 gram tin. Additions of 40 cc. of hydrochloric acid (sp. gr. 1.2). Temperature 200° C.

	Loss of Tin. Gram.
1st addition	0.1349
2d "	0.1792
3d "	0.4488
4th "	0.5117
5th "	0.5771
6th "	0.6154
7th "	No tin remaining in flask.

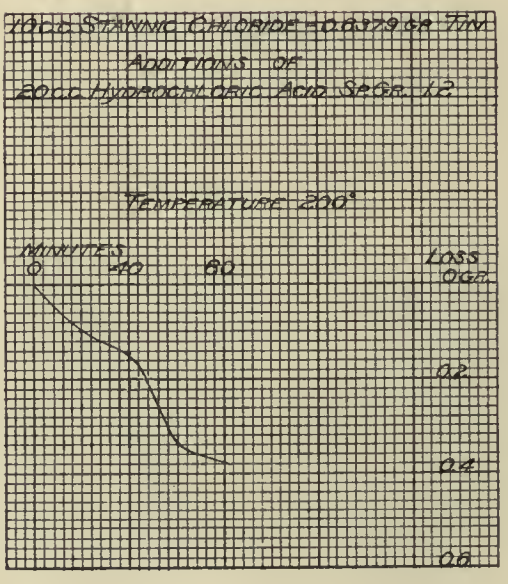
Time, two and one half hours; amount of hydrochloric acid, 280 cc.



In order to determine if smaller additions of acid would accomplish the same result, a series of evaporations was made like the foregoing, except that 20 cc. of acid were added instead of 40 cc.

10 cc. stannic chloride containing 0.6379 gram tin. Additions of 20 cc. hydrochloric acid (sp. gr. 1.2). Temperature 200° C.

	Loss of Tin. Gram.
1st addition	0.0999
2d "	0.1421
3d "	0.3392
4th "	0.3775

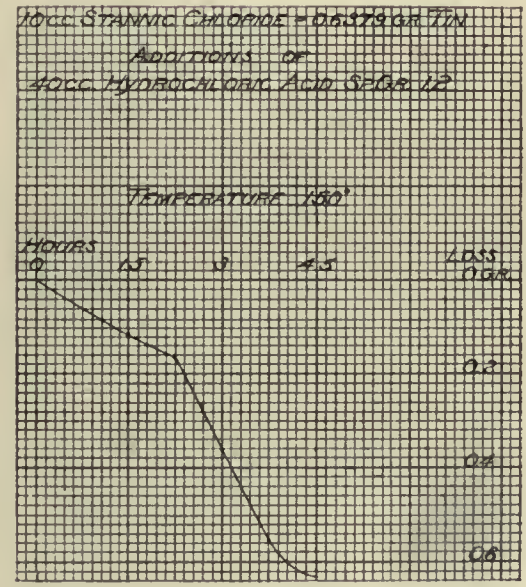


This experiment was not carried to the point of complete volatilization of the tin, since the results obtained showed that the operation would be much prolonged over the time found necessary in the previous experiment, in which 40 cc. of hydrochloric acid were used at each addition.

Nearly as satisfactory results were obtained at 150° as at 200°, as the following results show:

10 cc. stannic chloride containing 0.6379 gram tin. Additions of 40 cc. hydrochloric acid (sp. gr. 1.2). Temperature 150° C.

	Loss of Tin. Gram.
1st addition	.1130
2d "	.1607
3d "	.3629
4th "	.5520
5th "	No tin remaining in flask.



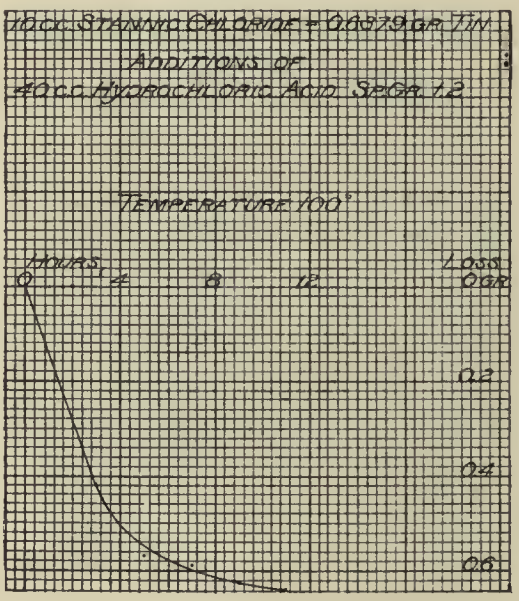
In this case the time required for complete volatilization of the tin was four and one half hours, nearly twice as long as at a temperature of 200°; but the amount of hydrochloric acid required was much less, namely 200 cc.

Experiments were next tried to determine how long a time would be required to volatilize the same amount of tin at the temperature of

the steam bath. These experiments were made in casseroles, and it may be that they are for this reason not strictly comparable with those made in flasks, on account of the possible condensation of the stannic chloride in the neck of the flask.

10 cc. stannic chloride containing 0.6379 gram tin. Additions of 40 cc. hydrochloric acid (sp. gr. 1.2). Temperature 100° C.

	Loss of Tin. Gram.	Time of Evaporation. Hours.
1st addition	0.4195	3
2d "	0.5665	5
3d "	0.5890	7
4th "	0.6214	9
5th "	No tin remaining in casserole.	11

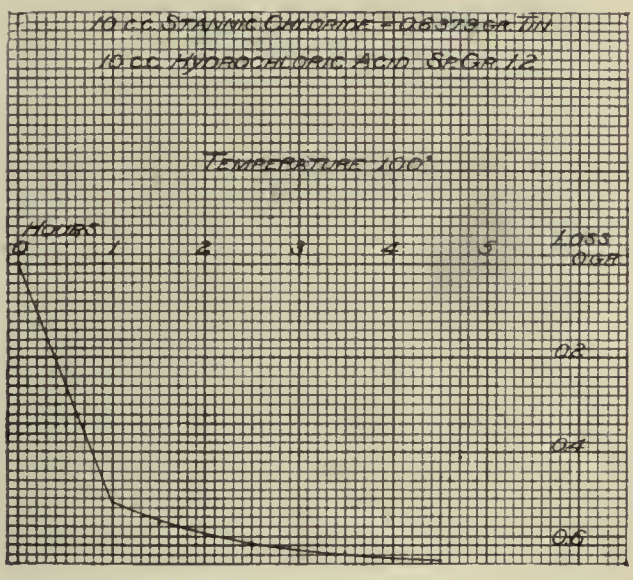


It thus took eleven hours under these conditions for the complete volatilization of 0.6379 gram tin at a temperature of the steam bath, although more than half of the tin was volatilized in the first evaporation when the solution of tin was concentrated.

All of the foregoing experiments point to the fact that the concentration of the solution of stannic chloride is a more important element in the volatility of the tin on evaporation than the amount or strength of hydrochloric acid present. In the following series of experiments the effect of concentration of the tin solution is still more strikingly shown:

10 cc. stannic chloride containing 0.6379 gram tin, evaporated to dryness, and then additions of 10 cc. hydrochloric acid (sp. gr. 1.2). Temperature 100° C.

	Loss of Tin. Gram.	Time of Evaporation. Hours.
1st evaporation	0.5097	1
1st addition of acid	0.5533	1½
2d " "	0.5738	2
3d " "	0.5963	2½
4th " "	0.6115	3
5th " "	0.6187	3½
6th " "	0.6300	4
7th " "	No tin remaining in casserole.	4½

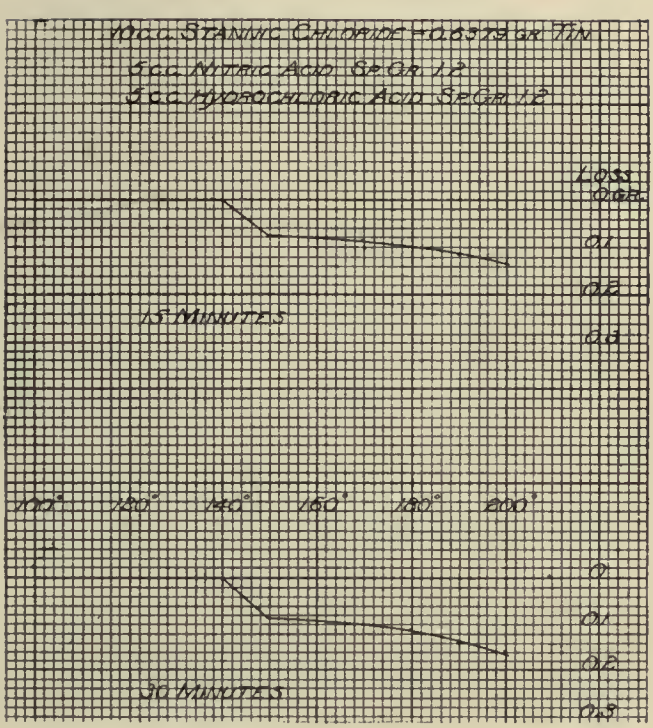


In each evaporation in this series the bulk of solution was only 10 cc., whereas in the previous series the bulk was 40 cc., and the time required for complete volatilization of the tin was thereby reduced from eleven to four and one half hours.

The effect of nitric acid on the volatility of stannic chloride was observed in one series of experiments. It tends to reduce the loss of tin, probably by the earlier formation of the hydrate. Following were the results obtained :

10 cc. stannic chloride containing 0.6379 gram tin, 5 cc. hydrochloric acid (sp. gr. 1.2) and 5 cc. nitric acid (sp. gr. 1.2).

Temperature. Centigrade.	Time. Minutes.	Loss of Tin. Gram.
140	30	None.
150	15	0.0792
150	30	0.0859
160	15	0.0814
160	30	0.0913
170	15	0.0859
170	30	0.1052
180	15	0.1011
180	30	0.1131
190	15	0.1177
190	30	0.1434
200	15	0.1320
200	30	0.1540



It will be noticed that in the presence of nitric acid not only does the loss of tin not begin until a temperature of 150° is reached, but that the difference between the fifteen and thirty-minute heating is much less than when hydrochloric acid only is present.

Finally, an attempt was made to make use of this volatility of stannic chloride in the separation of tin from other metals. Experiments were made with one metal only, namely, lead; and the separation was in this case quite satisfactory. To 10 cc. of the tin solution in a casserole was added a solution of lead chloride and 40 cc. of strong hydrochloric acid. This was evaporated to dryness. After four additions of 40 cc. of hydrochloric acid and evaporations to dryness, the tin was found to be completely volatilized. In four experiments the

amount of lead in the residue was found to be very nearly the same as the amount taken. Following are the determinations of the lead :

	Lead taken. Gram.	Lead found. Gram.
1	0.2324	0.2327
2	0.0942	0.0949
3	0.0897	0.0890
4	0.1794	0.1778

Massachusetts Institute of Technology,
June, 1892.

BOOKS RECEIVED.

Peters, Edward Dyer, Jr. Modern American methods of copper-smelting. Ed. 2 enl. x, 398 p. il. O. N. Y., Scientific Publishing Co., 1891. cl. $4.00.

Wyatt, Francis. Phosphates of America. Where and how they occur; how they are mined; and what they cost. Ed. 2. 187 p. il. O. N. Y., Scientific Publishing Co., 1891. cl. $4.00.

Hurst, George H. Silk dyeing, printing and finishing. viii, 226 p. 21 il. 11 pl. D. London, Bell (N. Y., Macmillan & Co.), 1892. cl. $2.00.

Landauer, J. Blowpipe analysis. Authorized English edition. By James Taylor. Ed. 2 enl. x, 173 p. 16 il. 1 pl. D. London and N. Y., Macmillan & Co., 1892. cl. $1.10.

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Nicholl, T. J., comp. Useful information and data for travelers, railway men and mechanics. Ed. 2. 69 p. T. Chicago, Danks & Co., 1892.

TECHNOLOGY QUARTERLY

AND

PROCEEDINGS OF THE SOCIETY OF ARTS.

VOL. V. OCTOBER, 1892. No. 3.

PROCEEDINGS OF THE SOCIETY OF ARTS.

THIRTY-FIRST YEAR, 1892–93.

THURSDAY, October 6, 1892.

THE 434th meeting of the SOCIETY OF ARTS was held at the Institute this day at 8 P.M., Hon. Jacob A. Dresser in the chair.

The records of the previous meeting were read and approved. Mr. George Wigglesworth, of Boston, and Prof. George R. Carpenter, of the Institute, were duly elected Associate Members of the Society. The Chairman then introduced Prof. William T. Sedgwick, of the Institute, who read a paper on the "Modern Scientific Views of the Cause and Prevention of Asiatic Cholera." The paper is published in full in the present number of the *Quarterly*. After an interesting discussion the meeting adjourned.

THURSDAY, October 27, 1892.

The 435th meeting of the SOCIETY OF ARTS was held at the Institute this day at 8 P.M., President Walker in the chair.

The records of the previous meeting were read and approved. The following papers were read by title:

"An Index to the Literature of Angelic and Tiglic Acids from 1842 to 1892," by Henry P. Talbot, Ph.D.

"Proposed New Formula for Yellow Pine Posts," by James H. Stanwood, S.B.

"Kaolin in Ashland, Mass.," by Frederick S. Hollis, S.B.

"The Insulation Resistance of Rail Circuits in Railroad Signaling," by Bertram H. Mann, S.B.

"On the Properties of Hydrogen Peroxide Solutions," by Henry P. Talbot, Ph.D., and Herbert R. Moody, S.B.

"The Volatility of Stannic Chloride," by Thomas M. Drown and George F. Eldridge.

These papers are published in Volume V, Nos. 1 and 2, of the *Quarterly*.

"On the Least Number of Vibrations Necessary to Determine Pitch," by Charles R. Cross and Margaret Maltby. This paper is published in the present number of the *Quarterly*.

The Chairman then introduced Dr. P. T. Burns, of the Institute, who read a paper on "Chemical Nomenclature," in which he discussed the reforms adopted at the International Congress at Geneva. The paper is published in full in the present number of the *Quarterly*.

The President extended the thanks of the Society to Dr. Burns, and declared the meeting adjourned.

CLEMENT W. ANDREWS, *Secretary*.

THE PHOTO-MECHANICAL PROCESSES.

By S. R. KOEHLER.

Read January 14 and 28, and February 25, 1892.

The photo-mechanical processes which are so largely employed at present for the production of printable pictures, to be used either in books and periodicals as illustrations, or independently for the portfolio or for framing, derive their name from the fact that the action of light (Greek *phos, photos*) on substances which are sensitive to it is called into play in the making of the blocks or plates from which the impressions are to be taken, and that these impressions are produced in the printing press, which is a *mechanical* contrivance. Their ultimate aim is to replace the older reproductive and interpretative processes by simpler, cheaper and more reliable methods. As compared with these older processes their modern rivals may be divided into two classes: *First*, those the final technical result of which is identical with that of their predecessors; *secondly*, those the final technical result of which is different.

To the *first class* belong those processes which reproduce relief engravings (wood-cuts and wood-engravings), or drawings specially made to imitate them; those which reproduce or imitate line-engravings, etchings, aquatints, mezzotints and other varieties of intaglio engraving, and those which reproduce or imitate planographic prints (lithographs and zincographs), executed either with pen and ink in lines or stipple, or with crayon, etc.

To the *second class* belong those processes which, although they produce relief blocks to be printed on the book press like ordinary wood-engravings, yet differ from the latter in the methods by which they arrive at the gradations from black through grays to white (such as the various grain processes, mezzotype, half-tone screen processes, etc.), but more especially the collographic process and the Woodburytype. The two processes last named are absolutely new, not only in this that they can work directly from nature without the intervention of the artist — a faculty which they share with the half-tone re-

lief processes, the photogravure (intaglio) processes and some of the photo-planographic processes — but also in the printing forms yielded by them, which are absolutely without analogues in the older reproductive processes.

To be able to understand the working of the photo-mechanical processes it is indispensable to have an understanding of the aim of the older processes, and of the means used by them to reach this aim, and we shall therefore have to devote a part of our time to their consideration.

The ultimate aim of all picture-printing processes is to produce blocks and plates which, when properly inked and pressed against a sheet of paper or other suitable material in a press, shall give a picture in which the gradations from black through the various shades of gray to white have the same relations, and therefore produce the same effect as in a black and white drawing made by an artist, either in lead pencil, charcoal, crayon, India ink washes, gouache or black and white oils. Three methods can be used to reach this end:

1. *Relief engraving*, such as wood-cutting and wood-engraving. Those parts of the surface which are to produce the picture are left standing in relief above the mass of the material used (commonly boxwood), while the whites between them which are not to print are cut away. If a roller charged with printing ink is passed over a block so treated those parts only which have been left standing in relief take up the ink. If a piece of paper is then pressed against the block the ink is transferred to it from the surface, and the result is an impression.

2. *Intaglio engraving*. In this case the lines and dots which are to produce the picture are cut into the plate used (intagliated), so that they form furrows and pits in it. The plates are usually of metal (copper, steel, etc.), and the furrows and pits are produced in them by cutting instruments (gravers, etc.), or by chemical means, that is to say, by corroding the metal by acids or mordants (etching). If a plate so treated is covered with printing ink and its surface carefully cleared so as to leave only the furrows and pits filled with it, and if then a piece of paper is pressed against it, the latter lifts the ink out of the furrows and pits, and the result is again an impression.

3. *Planographic methods*. These methods, best known as lithography (from *lithos*, stone) and zincography, are so called because the printing is done from perfectly plane surfaces, neither cut in relief

nor intagliated. The process is a chemical one. The picture to be printed is drawn on the stone or zinc with ink or crayon containing soap. The whole surface is then treated with certain chemical agents which decompose the soap, and at the same time attack the stone or zinc. The chemical action induced causes the fatty acids in the soap to enter into insoluble combinations with the stone or zinc. The printing surface now consists of two materials differing in properties. Those parts on which the drawing was executed are formed of an insoluble soap, which has the quality of shedding water, and therefore accepting fatty substances even after it has been wet; while the parts not drawn upon accept water readily, and therefore cannot take up fatty substances after they have been moistened. It follows that, if such a planographic surface is wet and then inked — printer's ink being a fatty substance — the drawing alone will accept the latter, while the parts not drawn upon will reject it. Naturally, if a piece of paper is pressed against a surface thus prepared it will take up the ink from the drawing, and once more the result will be an impression.

The principles underlying these three methods, *relief*, *intaglio* and *planographic*, will have to be borne in mind if the answer to the question next to be considered is to be understood. This question is: How can the methods under consideration be utilized to reproduce or imitate a picture made in black and white by an artist?

The means at the command of the artist are these: Black masses and white masses; black lines and dots; gray lines and dots of varying intensity or shade (value); and structureless grays, that is to say, masses of gray, either flat, but of varying intensity or shade (value), or gradated, and showing neither lines nor dots perceptible to the eye.

The *relief processes*, it is evident from what has been said, are *surface printing processes*, that is to say, the ink is taken up by those parts of the surface left standing in relief; and it may be said, therefore, with sufficient accuracy for the purposes of this discussion, that the impression produced upon the paper is uniformly black, or in other words that every line and dot in the impression is of the same value.[1] The limitations as well as the capacities of the relief processes are sufficiently apparent from this: Black masses are easily produced by allowing the surface to stand; white masses as easily by cutting it away. Black lines and dots evidently can be pro-

[1] The slight possibilities of variation offered by underlaying and overlaying are ignored here, nor can the difference between black line and white line engraving be considered.

duced with the same ease, but it is absolutely impossible to produce really gray lines. Such lines can, therefore, be only simulated by substituting for them narrower black lines, thus reducing the quantity of black and increasing that of white; or by white-lining a black line of the same width as the gray line, that is to say, breaking up the black line into a series of dots so that the proportion of black and white in it shall be the same as in the solid gray line of the original, and the effect upon the retina when the dotted line is seen at a sufficient distance to allow the black and white dots to blend shall also be the same. It follows from this that structureless grays are likewise beyond the reach of the relief engraver, and that he can only simulate them by masses of intermingled black and white lines and dots so proportioned that, by blending upon the retina of the eye when seen at the proper distance, they will produce the sensation of a shade of gray of the same value as the structureless gray of the original.

The *planographic processes*, being likewise *surface printing processes*, share, speaking generally, the limitations of the relief processes, and offer the same advantages, artistically considered. They can easily produce black masses and white masses and black lines and dots, but they cannot produce solid gray lines, nor really structureless grays. They can, however, simulate them more successfully, because they can produce, by means of crayons and otherwise, grains which are so delicate as to be almost imperceptible to the naked eye even at very short distances. For the production of crayon drawings the surface of the stone or zinc on which the drawing is to be executed is grained, giving it a texture somewhat like that of drawing paper. According to the fineness of this grain, and the quality — hard or soft — of the crayon used, the character of the drawing can of course be varied from extreme boldness to the utmost tenderness.

The *intaglio processes* are more favorably placed in this respect than either the relief or the planographic processes. We shall, however, be compelled to divide them into two groups, as they differ considerably in their capacities, and consequently in the results produced. To the first group belong line-engraving, punching, stippling, and etching in lines. These processes can easily produce white masses, simply by allowing the surface to stand. Black masses, on the contrary, can only be produced by laying black lines so closely alongside of one another that they run together in the impression. This brings us to the most important point in the technical character of all intaglio

processes. They are not surface printing processes, the ink being held by the cavities cut or bitten into the plate, and consequently they cannot produce absolutely unbroken black masses with the same facility as the relief and planographic processes. This is due to the necessity of cleaning the surface of the plate before the impression can be taken — an operation which is performed with rags or with the hand. If sufficient care is used the rag or the hand will not enter the lines, thus leaving them filled with ink while cleaning the surface. But if the attempt were made to produce a large, black spot, by hollowing out a correspondingly large spot in the plate, such a spot would produce only a gray smudge in the impression, for the rag or hand would enter it in wiping and would remove the ink from it. This explains why a black spot can only be produced by cutting a series of lines very closely together. The ridges left standing between the V-shaped furrows formed by the lines supply a support for the rag or hand, and thus prevent the wiping out of the ink. Gray lines may, of course, be simulated by narrow black lines, as in the surface printing processes. But the advantage which the intaglio processes have, because they are not surface printing processes, comes in here as an aid in the production of lines of different value. It is obvious that a line cut into the plate may differ not only in width, but also in depth, and that a deeper line will actually deposit more ink on the paper than a shallower one. Such lines may therefore differ in value in the impression, and thus the intaglio engraver who uses lines and dots has not only their breadth or size to aid him in producing the appearance of grays of differing value, but he can utilize, also, their varying depth.[1]

The processes which belong to the second group, such as mezzotint and aquatint, appear at first sight to be entirely independent of lines and dots, and capable of actually producing full black and structureless grays of every possible value. This, however, is not true, as the following consideration will show. Mezzotints are produced by first breaking up the surface of the plate into countless microscopic pits, separated from one another by equally minute ridges of metal, and then, by scraping, reducing the height of these ridges as needed. The breaking up of the surface of the plate — the laying of a mezzotint

[1] The aid to be derived from wiping and what is known as *retroussage* is ignored here as not properly the work of the engraver. The printer of process plates, moreover, can also make use of these devices, if they suit his purpose.

ground, as it is somewhat improperly called — is performed by an instrument known as the rocker. If a plate so grounded or rocked is inked, and then wiped and an impression printed from it, the latter will present one unbroken mass of velvety black, the minute cavities which hold the ink lying so closely together that the black dots produced by them run into one another, while nevertheless the ridges of metal between them form a sufficient support for the rag or the hand to prevent the wiping of the ink out of the cavities. The work of the engraver now consists in reducing the ridges between these cavities by scraping, as before stated, very slightly where there is to be a dark gray, more decidedly where there are to be lighter grays and removing them entirely where there are to be whites. By this scraping the ridges are reduced in height, and consequently the cavities between them in depth, so that as the scraping proceeds downward they hold less and less ink. At the same time, however, the tops of the ridges of metal — the ridges broadening downwards — become wider and wider, and therefore the black spots produced in the impression by the ink taken up from the cavities are separated from one another by more and more of the white paper seen between them. The various shades of gray in a mezzotint are the result, therefore, of two factors — the varying quantity of ink deposited on the paper according to the varying depth of the cavities, and the difference in the spacing of the dots produced. It is quite possible to lay a mezzotint ground so fine that it is difficult to detect the discontinuity of the grays with the naked eye; but it exists, nevertheless, as without the supporting ridges between the cavities, which form the ink-holding grain or tooth, all the ink would be wiped out of them. This is true, also, of aquatint, although in this case the cavities are the result of etching, and the grain produced is rarely so fine as to escape observation.

It is needless to tell you that these old hand processes — processes, that is to say, in which all or nearly all the work had to be done by the hand of man — have produced most admirable work, which will be the admiration of all ages to come. Nevertheless they are open to certain objections. But before I undertake to state these objections I wish to have it understood most distinctly that I am not going to act the part of the *advocatus diaboli*. On the other hand, I am not concerned at present with æsthetic theories or even fashions in art. My business is simply to give you historical and technical facts.

The first objection, then, to these old hand processes is their cost-

liness, a necessary result of the high class of artistic labor employed in the production of really good work, and of the length of time needed for engraving a block or plate, such time reaching from months into years, and the prices paid from hundreds into thousands. There is a tendency noticeable, therefore, throughout the whole history of the reproductive or multiplying arts to substitute the forces of nature for the work of the hand of man. The first effort in this direction is coeval almost with the very beginning of picture-printing, for etching, which seems first to have been practiced for the purpose we have in view towards the end of the fifteenth century, is, technically considered, nothing else but the substitution of chemical action for the work of the graver. The same is true, but in a still higher degree, of lithography, which was invented towards the end of the eighteenth century. It is the direct outcome of the desire alluded to; for although its invention may be called an accident, yet it must not be forgotten that Senefelder was consciously experimenting with the purpose of finding a cheap and easy substitute for the then known processes of engraving. Had that desire not been present the "accident" might have occurred a thousand times without leading to a result. The rapidity with which lithography spread over the world also shows that it met a long-felt want — a want which gives a distinctive character to the whole period from the end of the eighteenth to the middle of the nineteenth century.

This period may be called the era of "substitute processes," under which name I have grouped together all the methods devised prior to the introduction of the photo-mechanical processes, for producing printable blocks and plates with less labor, and therefore cheaper than the processes hitherto in use. The aim of all these processes was to get rid of the engraver — "the emancipation of the artist and the publisher from the engraver," to quote a euphemistic phrase then invented — or to enable the designer himself to be the engraver, by providing means for transforming the design into a printable block or plate by chemical or galvanic action, etc. To tell the bare truth about these processes they are, with isolated exceptions, the outcome of the industrial rather than of the artistic development of modern times. It is not their purpose to supply the artist with new means of expressing his individuality. They strive to imitate something more costly, by methods capable of rapid and extensive utilization in large industrial establishments. Cheapness is with them the first

consideration, with as much artistic perfection as this limitation will allow; and their career was only cut short by the introduction of the photo-mechanical processes, which served the same ends still better, and offered, besides, as we shall presently see, other advantages which were not offered by the substitute processes. Most of the latter have, therefore, gone out of use again, although some of them still hold their ground for certain classes of work, and, within certain limits, with excellent results. I allude here to the wax processes and kaolatype or clay surface processes (from *kaoline*, China clay), which may be summarily described as follows: Metal plates are covered with a thin coating of wax or of a clay preparation, and the design is cut into the coating with suitable tools so as to lay bare the metal. By electrotyping in the case of the wax processes, and by stereotyping in the case of the clay surface processes, blocks are obtained showing the design in relief lines, and these can be printed in the book press like wood-cuts. It is impossible to give a description here of all the processes which belong to this group;[1] but a word must be said concerning the relief etching process, not only because it was the earliest of its fellows, but also because the experience in etching gained by those who practiced it was a great help in the development of that form of the photo-mechanical processes most extensively used to-day. Relief etching is a substitute for wood-engraving, and is merely the reversed application of the etching process long known to painter-etchers and engravers. In this latter process the lines that are to print black are bitten into a metal plate, and those parts that are to show white in the impression are left standing. It is evident that in relief etching those parts which are to print black must be left standing, while those that are to show white are bitten away. To obtain this result different methods were essayed. The metal plate was covered with an etching ground, and the parts that were to show white were removed, or the design was executed on the plate in a fatty ink capable after it had dried of resisting the action of acids, or it was drawn on prepared paper in similar ink and was then transferred to the plate by pressure (in the press), the prepared paper being carefully removed after it had been wet from the back so as to dissolve or soften the preparation on its surface. This "transfer process" plays an important part, also, not only in lithography, but in some of the photo-mechanical processes, and it will be necessary, therefore, to

[1] For further details see the catalogue mentioned at the end of this paper.

keep it in mind. Of the etching involved — the biting away of the metal between the lines — it must suffice here to say that it is a much more complicated process than that employed by the painter-etcher or even the engraver. But of this more anon.

The objection to the old processes which the substitute processes sought to overcome was purely commercial — their costliness, and all they sought to do, therefore, was to reduce the labor of the engraver to a minimum, or at the very utmost to do away with it entirely. There was and is, however, a second objection which takes a higher ground — the unreliability of all hand processes considered from a scientific point of view. Necessarily when an engraver copies an artist's design, either from nature or a creation of his genius, it must undergo a transformation, owing partly to the technical limitations previously outlined and partly to the individuality of the engraver. In the case of a design from nature the influence of subjectivity is, indeed, still greater, for the transformation worked by the individuality of the engraver is preceded by that of the draughtsman, since it is a well-known fact that two designs by two different men from the same scene or object are never alike. We have, indeed, become much more skilled in copying than our predecessors used to be, so that copying might almost be called a modern art. To realize this it needs only to compare modern work of the same kind with say Rubens's copies from paintings and antiques, or the architectural etchings of the artists of past periods, or, still later, some of the reproductions of Mexican and Central American antiquities, which in many cases are little less than translations into a bastard renaissance style. But even with all our progress in this direction there are still so many possibilities of uncertainty that, with the awakening and growth of the modern scientific spirit, there awoke and grew, also, a desire for a process able to produce printable pictures — pictures, therefore, capable of wide distribution — which should be entirely free from the influence of subjectivity, and consequently absolutely correct scientifically, so far at least as form and light and shade are concerned. The reproduction of color has, indeed, also been essayed, but that is a subject which cannot be touched upon in these lectures.

Now it is evident that such a reproductive process must be absolutely independent of man, and must rest entirely upon the action of the unerring laws of nature. The outcome of the search for this desideratum was the discovery of the photo-mechanical processes and of

photography. In the mind of the public photography has become so closely identified with the usual form of portrait photography, etc., in which not only the printing form used (the negative), but also the so-called print (the positive, in the shape of silver prints, platinum prints, carbon prints, etc.), is the result of the action of light on substances affected by it, that it is looked upon as its starting point, while the photo-mechanical processes are supposed to be later applications of the principle involved. It is nevertheless true that the very first experiments made by Joseph Nicéphore Niepce aimed at the production of printable plates, and that the daguerreotype and photography on paper as we at present know it are later developments. The difficulties besetting the production of printable plates were so great, however, that the experiments made in this direction were obscured by the splendid results achieved by sun-printing, and were only prosecuted with renewed vigor — although never wholly abandoned — when it became evident that the products of photography, pure and simple, were too expensive and too unstable to supply the place of pictures in printer's ink printed on a press.

The aim followed in these researches — the discovery of a scientifically reliable reproductive process — is very clearly expressed in the announcement of the prize offered by the Duc de Luynes in 1856. "It is," says this announcement, "with a view to hastening the moment so much desired, when the processes of printing or of lithography shall permit the reproduction of the marvels of photography *without the intervention of the human hand in the design*, that M. le Duc de Luynes . . . has established a prize." These hopes were based on the assumption that photography was actually scientifically reliable. It is hardly necessary to say that this illusion has long since been dispelled, and that we are to-day fully aware that there are a thousand and one influences at work which make the result of photography doubtful, even if we leave out of the calculation the errors of judgment introduced by the still unavoidable intervention of man, not only as a chemist and physicist (optician), but to some extent also as an artist (in the choice of pose, point of view, lighting, etc.). But even if we had a method eliminating all these sources of error — a sort of "drop-a-nickel-in-the-slot-process," in which absolutely everything is done by nature — there would still be varying conditions of light, of moisture, of temperature, of electricity, which would affect the result, and it is probably vain to hope that these difficulties will

ever be completely overcome, or their causes brought under control. In spite of all these limitations, however, it remains true that on the whole photography is, scientifically considered, the most reliable delineator at present at our service. If we add to this the commercial advantages offered by it, its rapid development and wide diffusion are easily explained, while at the same time we may confidently assume that the end is not yet, and that a long line of development and improvement is still before us. With all these shortcomings, therefore, present to our mind, we shall be unable to deny that the photo-mechanical processes represent a vast step in advance on the road towards the desired aim, which is nothing less than the elimination, not only of the *engraver* — whom the substitute processes had already sought to oust — but also of the *designer*, from the production of printable blocks and plates. To take any object, whether artificial or natural — a work of art, a scene in nature, animals, the human face and figure — and to produce therefrom, solely by the agency of nature, a printing form from which an unlimited number of impressions can be taken in ink as unaffected by the ravages of time as that used for the printing of blocks and plates engraved by hand — this is the aim of the photo-mechanical processes.

If we ask now what are the agencies by which the engraver and even the designer — the "artist," that exalted personage, to whom we are accustomed to look up to as a superior being more highly gifted than his fellows — are to be displaced, the answer may perhaps be something of a shock to delicately strung natures. There is, indeed, the glorious light, whose life-giving, silent, mysterious influence is called into play; but the bodies upon which it acts, and with which we shall have more tangible dealings, are of a much more terrestrial, lower and vulgar order, to wit: *pitch*, *glue* and some *poisonous chemicals*. What mean substances! What an opportunity to moralize upon the exaltation of the little, or to preach upon the text that the stone rejected by the masons shall become the corner stone of the edifice! For if the engraver, and even the *artist*, are to be discharged to make way for pitch and glue, and are to be told that these despised bodies, which one cannot touch without soiling one's hands, are more reliable under the influence of light than their brains and hands under the same influence, what may not be in store for the rest of us?

Formulating the statement just made more concisely we may say that the materials most generally used in the photo-mechanical processes are resinous substances, especially *asphaltum* (bitumen of Judæa, Jews'

pitch); glutinous substances, more especially *albumen* and *gelatine;* and of chemicals, *potassium bichromate* and *ammonium bichromate.* Other substances have been tried, but those named may be said to be the only ones now practically in use; and we shall therefore confine ourselves to their consideration. The properties possessed by them which come into play in the photo-mechanical processes, and the possibilities which grow out of them, may be outlined as follows:

Asphaltum is a mineral resin of varying constituents found in Trinidad, on the shores of the Dead Sea, etc. It is soluble in benzine, benzole and turpentine. Exposed to light it undergoes what is apparently an oxidizing process, and becomes insoluble in its ordinary solvents. If, therefore, we coat a metal plate with asphaltum and then expose it to the action of light in parts, while other parts are protected from this action, as for instance under a negative, the result will be that after the exposure the coating is no longer uniform, but consists of asphaltum in two distinctly differing conditions, those parts of the coating which were protected from the light being still soluble in benzine, etc., while those which were exposed to its action have become insoluble. We can now wash away these soluble parts and can then etch the plate, the insoluble asphaltum left on it serving as an etching ground, or "resist," that is to say, protecting the parts covered by it from the action of the mordant. As not all the constituents of crude asphaltum are equally sensitive to light, it is purified for photo-mechanical purposes by treating it with ether.

Albumen, or white of egg, is soluble in cold water, not only in its fresh condition, but even after it has been dried. If, however, it is mixed with potassium or ammonium bichromate, dried, and exposed to light, it becomes insoluble and capable even, to a certain extent, of resisting acids. If, therefore, we coat a plate with a solution of albumen and a bichromate, dry it in the dark and then expose it to the action of light, while parts of it are shielded from this action, as under a negative, the result will be the same as in the case of asphaltum. We shall have on the plate albumen in two conditions, soluble and insoluble; and after the former has been washed away the plate can again be etched, the insoluble albumen acting as an etching ground or "resist."

Gelatine is best known to most of us as a material for delicate puddings. According to Professor Eder, who has made a special study of the subject, the term is used to designate "different kinds of

pure glue without, however, applying to a particular species. In glue we distinguish *glutine* (bone glue, made from leather, parchment, bones and isinglass) and *chondrine* (glue made from the cartilages of the ribs and joints). The properties of glue vary considerably, according to the preponderance of glutine or chondrine. Neither the one nor the other is found in commerce in the pure form ; but nearly all ordinary commercial gelatines, and also most of the photographic gelatines (*i.e.*, those kinds specially made for photographic purposes), consist essentially of glutine." According to the same authority the chemical constitution of these organic substances is as good as unknown; that is to say, we know that they consist essentially of carbon, hydrogen, nitrogen and oxygen, but it is impossible to fix the proportions of these elements. Their action, however, under the influence of light in the presence of certain chemicals, is sufficiently understood; and upon this action are based the uses made of gelatine in the photo-mechanical processes.

Gelatine absorbs *cold* water, and consequently swells up when treated with it. In *hot* water it dissolves. It loses these properties, however, when exposed to light in the presence of a bichromate. The bichromates in contact with organic substances part freely with one equivalent of chromic acid under the influence of light, and the acid again is easily deprived of part of its oxygen and reduced to oxide. The ease with which chromic acid is thus decomposed is illustrated by bringing it together with alcohol. The deoxidation of the acid and the oxidation of the alcohol take place so rapidly that the latter ignites. According to Abney, when gelatine mixed with potassium bichromate is exposed to the action of light, the result is a loss of hydrogen and a gain of oxygen in the organic matter, and the formation of potassium hydroxide and chromic oxide. It would seem, however, as if the action could hardly be so direct. A film of sensitized or bichromated gelatine — of gelatine, that is to say, dissolved in hot water and mixed with a solution of a bichromate — spread on a glass plate and dried in the dark is of a brilliant orange color. Upon exposure to light for a sufficient length of time it assumes a beautiful dark brown hue, said to be due to the presence of chromium chromate. This brown color disappears after copious washing in cold water, and the color that remains permanently is of a pale greenish tinge, caused by the presence of chromic oxide, and similar to the color of ordinary green glass bottles, which is also due to chromic oxide. The action of light on the bichromates in the presence of organic matter can also be illustrated by means of paper

soaked in a solution of potassium bichromate, and it has been used for the production of a kind of sun-prints called *chromotypes*. If a sheet of such paper, after it has been dried in the dark, is exposed to light under a negative the picture is brought out in brown on an orange ground. Such a picture, however, is not permanent, as on further exposure to light after the removal of the negative the orange ground would also turn brown. To "fix" it, it is necessary, therefore, to wash out the unchanged bichromate; but this affects also the brown portions, and the picture is finally left in a greenish tint on white paper. Experiments like these, made by Mungo Ponton and published by him in 1839, were the starting point of all the processes based on the use of bichromated gelatine, although it was left to Becquerel to demonstrate, in 1840, that it was really the sizing matter of the paper which, in conjunction with the bichromate, produced the effect, and to Fox Talbot, in 1852, to investigate still further the action of the bichromates on organic substances under the influence of light.

The behavior of bichromated and unbichromated gelatine after exposure to light, and in the presence of hot and cold water, can readily be illustrated by means of the sheets of gelatine sold by dealers in photographic materials. If small pieces of such sheets are soaked in a solution of a bichromate, so that one half is sensitized (bichromated), and the other half left in its natural condition, and then exposed to the light, and the half which has turned brown washed out until it assumes the greenish color, it will be found that upon immersion in *cold* water the part left in its natural condition will soften and swell, whereas the bichromated portion remains hard and leathery, and does not swell up at all, or at least very much less than the other half. This experiment may be repeated over and over with the same piece. If, however, a sheet of gelatine so prepared is thrown into *hot* water, the half of it which is in the natural state will dissolve, while the bichromated part remains hard and leathery, as before.

A number of possibilities which have been utilized in the photomechanical processes grow out of the behavior of gelatine under the conditions just described: —

A film of bichromated gelatine may be exposed under a negative, and then treated with cold water. It is evident from what I have told you that the unexposed parts will swell up above the exposed parts, which have lost the faculty of swelling, thus forming a relief in soft gelatine.

Such a film, after exposure, may be treated with hot water, which will dissolve out the unchanged gelatine between the parts that have been changed, and consequently hardened. The result is a relief in hardened gelatine.

The two possibilities so far stated are easily apparent from the experiments described. A number of others have been found, however, which do not follow so self-evidently: —

A bichromated film may be so treated that after it has been exposed and then soaked in water it will accept printing ink on the exposed parts, the amount of ink accepted varying with the amount of exposure, while the unexposed parts, which have retained all their capacity for absorbing water, will reject it. It is evident that such a film acts like a stone with a lithographed drawing on it.

A film treated as described in the preceding paragraph shows a very fine reticulation or vermiculation on its surface. This "grain" can be made larger and more apparent, its closeness and size varying with the varying amount of light that has acted on the surface. Such a surface can be rolled up in ink, and transfers from it made to stone or metal.

A bichromated gelatine film, after full exposure to light, becomes impermeable to acids, while in those parts in which it has not been fully exposed the permeability varies in inverse proportion to the amount of exposure. It can, therefore, be used as a "resist" in etching processes.

Finally, bichromated gelatine, after exposure to light, shows a capacity for resisting the impact of the sand blast, the resistance varying with the amount of exposure. Upon this observation is based a photo-lithographic process.

We are now in a position to consider the various applications which can be made of the possibilities offered by asphaltum and bichromated albumen and gelatine. But as a matter of course we shall have to confine ourselves to the leading processes only, and more especially to those which are in practical operation to-day. And it will be well to state here, also, once for all, that only the leading principles can be touched upon. The innumerable great and small difficulties which beset the path of the photo-mechanical process man and make his lot anything but a happy one, but upon the successful overcoming of which, nevertheless, depends the commercial availability of his labors, must be ignored.

The first point to be considered is the character of the originals to be reproduced. These may be of two kinds: *First*, in lines and dots on a light ground, such as pen and ink drawings on white paper; and *second*, washed drawings, paintings and photographs from nature which show structureless grays. Originals of this second kind are commonly called "half-tone originals," and by this name we will designate them in our further discussion.

To begin with we will take the simplest problem that can be proposed — the making of a relief block, to be printed in the book press, from a drawing in black lines and dots on white paper. The first care of the operator will be to examine the drawing. If it shows gray lines and dots intermingled with the black he will, if it be at all feasible, reject it, as it will be impossible to get a perfect result from such a drawing. The reason will become apparent to you, if you will recall to yourselves the necessities of the surface-printing processes, which cannot, as we have seen, produce actually gray lines, but can only simulate them by substituting either finer black lines or lines broken up into dots by cutting through them (by white lining) for the gray lines of the original. This, however, the photo-mechanical process presently to be considered cannot do. To insure a perfectly satisfactory result a black and white negative is needed, that is to say, a negative consisting of lines showing the clear glass without any veiling or obscuration whatever on a black ground of perfectly even density. It is obvious that a negative made from a drawing in which there are gray lines cannot fulfill these conditions, as some of the lines in it will be more or less veiled; and the result in the block produced will be either solid black lines where the gray lines should be, which will be too dark; or rotten lines, which will be ungainly; or no lines at all, which will give to the picture a spotted appearance, with contrasting black and white masses and no intermediate tones to unite them. A drawing for reproduction ought, therefore, to be executed on white paper in lines and dots of absolute black, varying only in breadth or size and in spacing.

Supposing the operator to be in possession of such a drawing he will have the choice of three processes now more or less generally in use, for its reproduction, to wit: the *etching process*, the *swell-gelatine process* and the *wash-out process*. He is most likely to choose the first, and we will therefore begin with its consideration.

There are two varieties of the *etching process*, according to the

nature of the substance used to produce the "resist," which substance may be asphaltum or bichromated albumen. Either will do, although asphaltum will perhaps give a somewhat sharper result. But as the albumen is a time-saver it is generally given the preference. In either case the first care will be to make a black and white negative from the drawing, generally somewhat reduced in size. This negative will have to be reversed, that is to say, instead of showing the right to left and the left to right, as in an ordinary negative, the right in it will have to be to right, and the left to left. The necessity for this will become clear when the various changes of position are considered which the subject has to undergo in the process. The original is right-left; an ordinary negative would be left-right; the design on the block from such a negative would be right-left, and the impression would consequently be left-right. To prevent this the negative is made right-left, the same as the original. The block made under such a negative will as a matter of course be left-right, and the impression, consequently, right-left, as it ought to be.

A metal plate, generally a zinc plate, is now coated very thinly and evenly with a solution of purified asphaltum in benzole, or with a solution of albumen sensitized with either potassium or ammonium bichromate, and after it has been dried in the dark it is exposed to daylight or electric light under the negative. It is obvious that the light can penetrate only through the clear spaces of the latter, and, as these spaces represent the lines of the original drawing, that the result will be a reproduction of the drawing on the plate, in either hardened asphaltum or hardened albumen, while the spaces between the lines will be left in their soluble condition. The exposure having been completed the next step will be the "development" of the image, or in other words the removal of the soluble parts from the plate. If asphaltum has been used this "developing" is accomplished by washing with turpentine. If albumen has been used the plate is first rolled up in a special kind of fatty ink (etching ink), so that it is uniformly black all over, and the "developing" is done with water. In either case the drawing is left on the plate in lines covered with a material capable of resisting acids, upon a ground of bare zinc. The plate is now in a condition to be etched into relief, for which purpose nitric acid diluted with water is used. After the etching has been completed it is generally necessary to trim the plate with the graver, *i.e.*, to make corrections and add refinements by reducing lines that

are too thick, or to use the burnisher for the opposite purpose, and to rout out the large white spaces which it would be too tedious and costly to etch deep enough. The plate is finally made type high by mounting it on a block of wood like an electrotype, and it is then ready for printing from it in the type press, as from any other block engraved in relief.

I must put in a word here about the difficulties of etching as applied in these processes, which, as I told you before, demand of the etcher an extraordinary amount of skill and care. It will not do, as in the case of ordinary etching, to bite, take a proof and then if need be lay a re-biting ground merely on the surface and bite again. Not only the surface of the lines must be protected, but their sides as well, and this in corresponding progression downward as the biting progresses in depth. To this end the plate is rolled up with "etching ink" between the various stages of the etching, and powdered with resinous substances, commonly dragon's blood. The plate is then heated, so as to cause the ink and the dragon's blood to melt and gradually to coat the sides of the lines by flowing downward, first filling up the finest parts of the work, then the next finest, and so on until only the coarsest parts are left open for a final etching. In this way is secured not only sufficient depth between the lines to prevent the ink from filling up the block, but also a "foot" for each line, that is to say, increasing thickness downwards, however delicate the line may be on the surface, so as to prevent its breaking under pressure. This species of etching was first practiced, as before noted, by the substitute process workers, and as it is said to have been more especially perfected by Gillot, of Paris, it used to be and still is occasionally called *gillotage*. The delicacy of this work can best be estimated by examining a half-tone block.[1]

The introduction of the electric light has made it possible to use the process here described, with albumen as the basis of the "resist," for quick newspaper work, which would be out of the question if sun-

[1] A process is now coming more and more into use which does away with many of the difficulties of etching. The "resist" used is still held a secret, but is said to consist of bichromated albumen, mixed with other substances. It is laid on a copper plate and baked, which so hardens it that it resists acids without rolling up and powdering with resinous substances, and can be removed only by grinding. The nature of the copper and that of the mordant used, *i. e.*, perchloride of iron, make it possible to get sufficient depth and sufficient resisting power in the lines left standing, by one etching, at least for ordinary purposes. It is stated that this "enamel resist" cannot be used on zinc.

light were a necessity. The illustrations in the daily papers, whatever we may be tempted to think of them from other points of view, are certainly marvels of science applied to industry — a fact which the public to whom they are presented at the breakfast table does not sufficiently realize. An accident happens at noon; a small photograph is taken of the scene. This is enlarged, a drawing made over the enlargement, the photograph bleached out under the drawing, a negative made from the drawing, a plate coated, exposed, developed, bitten, trimmed, stereotyped and blocked, put on the press about midnight and a hundred thousand impressions or so distributed by six o'clock next morning. We are so surrounded by wonderful things that they cease to be wonderful. But it is well now and then to force ourselves to stop and think at least for a moment.

As the second possible method for the reproduction of our drawing I named to you the *swell-gelatine process.* In this case we shall need an ordinary — or as the professional slang has it, a "straight" — negative, that is to say, one that has not been reversed. From what follows, aided by the application of the right-left, left-right process, you will be able to reason out why. The first step in the swell-gelatine process is to lay a thin film of bichromated gelatine on a glass plate and to dry it in the dark. It is then exposed under the negative, the result being that those parts of the film which were acted upon by the light, and which correspond to the lines of the drawing, are hardened and lose their capacity of absorbing water and swelling, while the parts between the lines retain this capacity. It follows that, upon soaking the film in water after the exposure, those parts which were protected from the action of the light, and which correspond to the whites of the design, swell up, while those which were not protected, and which correspond to the blacks of the design, do not swell. The film in this condition represents a mold or matrix, and a cast taken from it could be used in the printing press, if it were of a material capable of withstanding the pressure. As this is not the case, however, a second matrix has to be made from the first cast, and from it a stereotype is obtained, which is finished with the graver, etc., and routed where necessary, and then made type high by mounting it on a wood block. All the casting may be done in plaster; but the first cast is generally made in a soft mass, the composition of which each operator tries to hold secret. The swell-gelatine process, although patented by

Poitevin, in 1855 (see his English patent of that year, No. 2,816), was specially brought to perfection by John C. Moss, of New York.[1]

A third possibility, finally, is found in the *wash-out process.* For use in this process the gelatine is first rotted, that is to say, kept in solution at a high degree of heat until it begins to decompose. It is an unfortunate characteristic of all so-called "practical" books never to give reasons or indulge in theoretical explanations. In accordance with this characteristic none of the books that I have met with on the subject give any reasons for this rotting, and I have not as yet been able to make experiments in this process myself. It appears, however, that the rotting facilitates the washing out, without incapacitating the gelatine for setting. A thick film of this rotted gelatine, sensitized by admixture of a bichromate, is formed on a plate of glass, from which it is removed after it has set. The side which was next to the glass, and which is therefore the smoothest, is then exposed under the negative, which in this case must again be reversed. The exposure hardens the film under the clear spaces of the negative which represent the black lines of the original, but leaves it soluble under the dense parts, which represent the whites. The film cemented to a zinc plate is now treated with warm water for the purpose of washing away the unchanged gelatine between the hardened lines, leaving the latter standing in relief. As soon as sufficient depth has been obtained the washing away, or "development," is stopped. A film of this kind can be printed from, and this is actually done in the "glue-type" process. Usually, however, a wax mold is made from the film, and from this an electrotype.

The *planographic processes* for the reproduction of drawings in black lines, that is to say, those processes which produce designs on stones or zinc plates, printable in the lithographic press like ordinary lithographs or zincographs, are of two kinds, to wit: *direct processes*, which produce printable pictures by the action of light directly on stones or zinc plates; and *transfer processes*, which involve the making of a picture on paper by the action of light, and a transfer from this to stone or zinc. As the transfer processes are the only ones now in practical use we shall confine ourselves to them, and indeed to only one of them — the Osborne process, invented by J. W. Osborne, August 19, 1859, and patented by him in Victoria, September 1,

[1] Since these lectures were delivered John Calvin Moss, born in Pennsylvania in 1838, died in New York, April 8, 1892.

1859. It was the first process of its kind of practical value, and the changes introduced since its invention affect only matters of detail. A sheet of paper, coated with albumen and gelatine sensitized by a bichromate, is exposed under a reversed negative. After the exposure the sheet is covered over its whole surface with lithographic transfer ink. In this condition it is floated on hot water to coagulate the albumen, and it is then "developed" with cold water. The cold water causes the gelatine on the sheet to swell, in which condition it has practically no affinity for the ink, while the wet sponge with which the sheet is rubbed for the purpose of developing it has a very great affinity for it. The sponge, therefore, removes the ink from the parts between the lines, but it does not remove it from the lines themselves, as the hardened gelatine which forms their basis has as much as or even greater affinity for it than the sponge. The office of the albumen is to hold the photographic image, after coagulation, with comparatively short exposure, to protect the paper during the development, and to hold the sheet on the stone during the transfer. Upon the completion of the development we have a drawing in transfer ink, that is to say, in an ink containing soap, on paper, and this drawing can be transferred to stone or zinc in the manner familiar to all lithographers, *i.e.*, by laying it down on a stone or zinc plate face downward, pulling the two through the press together, wetting the paper from the back and carefully removing it, which will leave all the ink on the stone or zinc with scarcely a trace remaining on the paper. The stone or plate is now prepared in the usual manner, with acid and gum, and the result is a true lithograph or zincograph, differing in nothing whatever from a lithograph or zincograph made with lithographic ink and a pen.

As before stated, drawings in absolutely black lines on white paper, yielding a perfect black and white negative, are a necessity for all the processes so far examined, if the results are to equal the originals. Very admirable results have, indeed, been obtained from pencil drawings, and other drawings with gray lines, in cases in which an artistic indication is valued above mechanical perfection. But such perfection is often necessary, and still oftener the wish of the parties for whom the work has to be done. As, however, artists' drawings of the usual kind rarely if ever meet the requirements of the processes, these latter have given rise to a class of work specially known as "process drawing," and have brought into use a number of devices, such as

grained and printed papers of a great variety of patterns, scratch paper, shading mediums, pasting tints, etc., altogether too numerous to mention here in detail, and worthy of special study. It cannot be denied that much of this kind of work so far done is hideous; but it is quite within the possibilities that it should be good and even refined, provided the right sort of designers will take hold of it in the right spirit.

In view of what I have told you so far, you will find it easy enough now to understand the making of a *photogravure*, that is to say, of an intaglio plate, from an original in black lines on white paper. All we shall have to do will be to use a straight black and white positive (a transparency) instead of a black and white negative under which to expose our plate coated with asphaltum or bichromated albumen. Under such a positive the black lines of the design will be protected from the action of the light, whereas those parts which represent the white paper on which the black lines are seen will be exposed to it. It follows that those parts of the sensitive film on the metal plate which represent the lines remain soluble, whereas those which represent the ground or white paper are hardened. The development will consequently lay bare the metal along the lines of the design, while it will leave the resist intact on all other parts of the plate, precisely as if the lines had been drawn through an etching ground with a steel point, so that they can be bitten in with a mordant in the usual manner. The process is, in fact, simply a reversal of the relief-etching process.

The use of asphaltum on a metal plate, exposed under a positive for the purpose of producing intaglio printing plates, forms the starting point of the history of the photo-mechanical processes and of photography. Joseph Nicéphore Niepce began his experiments in this direction in the year 1813; but he used crude asphaltum with oil of lavender as a solvent, and prints or designs on paper, made transparent, as printing screens (positives). An impression from one of his plates, made in 1824, and still preserved in the museum at Châlons-sur-Saône, can be seen in the United States National Museum at Washington. So far as known this plate is the oldest specimen of photo-mechanical process work now extant.

The fact, however, that the intaglio processes can produce, as we have seen, not only black lines but also lines of other values — a fact upon which depends much of their attractiveness — makes the frequent

reproduction of works in purely black lines by photogravure somewhat improbable, and as the processes so far described are incapable of anything else we shall now have to consider those means which can give us gray as well as black lines. It is said, indeed, that Amand-Durand, of Paris, who is famed the world over for his beautiful reproductions of the engravings and etchings of the old masters, uses the primitive asphaltum process. If this statement is true it explains the very careful and elaborate retouching with the graver which his plates show, the photo-mechanical part of the work serving evidently only as a basis for that of the engraver. But while this retouching does not in any way detract from the general beauty of the result it utterly destroys its scientific value; so that it would be quite impossible, for instance, to rely upon an Amand-Durand reproduction for the identification of an old engraving of doubtful authenticity.

The problem, then, which next presents itself to us is this: How can black lines and gray lines be produced by photographic means on a metal plate, which is to be printed on a roller or copper-plate press like any other intagliated plate? It is clear that such lines must differ not only in breadth but also in depth, becoming narrower or shallower, or both, in accordance with the shade of gray which they are to produce in the printing.

Poitevin proposed the *swell-gelatine process* for this purpose, with a positive, reversed this time, instead of a negative. With a "black and white" positive, like the negatives with which we have dealt so far, those parts of the film protected from the light, having been fully protected throughout, would swell to even height; and an intaglio plate made from a relief thus obtained would show lines of equal depth throughout, and hence printing equally black throughout. But the case is different with a positive which is not merely "black and white," but which shows black only where there is actual black in the original, and varies in density where there are grays of varying value. Now as the power of the gelatine film to absorb water, and consequently to swell, will eventually vary in inverse ratio to the amount of light that has acted upon it, it follows that it will retain all its capacity of swelling where it was entirely protected from the light, and that this capacity will decrease with the increase of the amount of light which has passed through the less and less dense portions of the positive until where the light passed in full force all the swelling capacity will have been lost. A bichromated gelatine film exposed under such a positive and then

swelled will consequently present lines in relief, the height of which must differ in inverse ratio to the amount of light that has acted on them. A plate made from such a relief will, therefore, show lines which are deepest where they are to print blackest, and which decrease in depth where they are to produce lighter values, the breadth at the same time being regulated by the breadth of the lines in the original.

Another possibility grows out of the fact before noted, that *bichromated gelatine becomes impermeable to mordants in proportion to the amount of light that has acted upon it.* That is to say, where no light has affected it, it allows the mordant to pass most freely; where it has undergone the full effect of the light, it allows none to pass; and the gradations between these two extremes show corresponding degrees of permeability. Hence, if we expose a bichromated gelatine film on a metal plate under a positive — unreversed in this case — and then expose this film to a mordant (under conditions to which I shall have to refer later), the action of the latter will differ along each line in accordance with the variations in permeability produced; and the result will be lines bitten into the plate varying not only in breadth but also in depth, and consequently in ink-carrying capacity.

Quite different from any of the methods hitherto mentioned is the one introduced by Scamoni, of St. Petersburg, and named by him *helio-electrogravure.* It is based upon the fact that collodion negatives, and consequently also collodion positives, on glass show a perceptible relief. Such a positive, made from an engraving with collodion of a special quality, is so treated as to increase the relief of the lines as much as possible, and a plate is then made from it by electrodeposition. Considerable hand work is necessary, however, to finish the plate.

What may be termed a modification of the *wash-out process* has also been used for the reproduction of line work intaglio plates. A relief consisting of lines in hardened gelatine, differing in height according to the amount of light that has acted on them, is formed on a metal plate, and an electrotype made from this relief furnishes the printing plate. The process will be found described in Husnik, *Heliographie,* second edition, Vienna: 1888, page 60, etc. It cannot be understood, however, without some knowledge of the pigment or carbon printing process, to which I shall have to call your attention later on.

We have so far confined ourselves entirely to the methods used in the reproduction of *line work.* The rest of our time must be given to

the study of the processes employed for making printable blocks and plates from *half-tone* originals, that is to say, originals which show structureless grays in flat as well as in gradated masses, such as India ink drawings, black and white gouaches and oils, water colors, oil paintings, and lastly, but certainly not least, photographs from nature. It will be best in this case to begin with the *intaglio processes*, not only because they are the first historically but also because they are more easily understood than the planographic and relief processes. It would be very interesting to indulge in an historical survey, and to examine Niepce's experiments with asphaltum ; Pretsch's photo-galvanographic process, which involved the reticulation of gelatine, etc.; but we must restrict ourselves, as the subject is too vast, and even the examination of the methods generally used at present is likely to claim more time than you have allotted to me.[1]

The methods just alluded to depend on the varying degrees of permeability retained by a film of bichromated gelatine spread on a metal plate after exposure under a half-tone positive. The comparative resistance to the action of a mordant of the various parts of such a film along a straight line may be represented by a curve, the highest parts of which indicate the points of greatest resistance, while the lowest correspond to those of least resistance. It goes without saying that the action of the mordant through such a film will reproduce the curve in the copper plate, that is to say, at the point of greatest resistance the plate will not be attacked at all ; at the point of least resistance it will be attacked most vigorously, and therefore bitten away most deeply ; and the various degrees of resistance between these two extremes will be represented by cavities in the plate, varying in depth with the degree of resistance. The bitten plate, therefore, contains the original picture transformed into a mold for a relief, the deepest parts of which correspond to the blacks, the less deep parts to the grays and those parts which have no depth at all, that is to say, where the surface of the plate has been left intact, the whites of the picture. Now if we can fill this mold with printing ink, and then transfer the relief in ink thus formed to paper, we shall have a reproduction of the original in all its gradations from black to white. This is impossible, however, as we have seen, owing to the necessity of wiping the surface of the plate in order to clear the whites. The cavities which hold the ink being very shallow and broad, the hand or the rag with

[1] Some details are given in the catalogue mentioned at the end of this paper.

which the wiping is done cannot help entering them, and therefore removing the ink from them. It follows that the method is useless unless a grain or tooth can be given to the plate which, while it does not interfere with the gradations needed, will yet form a support for the hand or rag in wiping, and will thus enable the plate to retain the ink while its surface is being cleared. Various methods have been proposed and used for the production of this grain or tooth.

The first of these methods may be called the *photogravure screen process.* If we take a transparent screen of some kind which will transmit the light at regular intervals and exclude it between these intervals — a piece of woven stuff, for instance, or best of all a glass plate upon which have been ruled very fine black lines parallel to one another or crossing one another — and expose under it a metal plate coated with bichromated gelatine, the result will be that all those points of the film acted upon by the light traversing the openings of the screens will be hardened and become impervious to fluids. If we now expose a plate so prepared to the action of a mordant, this latter will bite through those parts of the film which have remained soft, and consequently will form pits in the plate; whereas it cannot act on the plate where it is protected by the hardened parts of the film, and the corresponding parts of metal will therefore be left standing in relief. Hence we have now a plate which consists of minute ridges of metal, even at their tops with the surface of the plate, with minute depressions between. If we ink this plate, then wipe it and take an impression from it, we shall find that it has held the ink very well, but that the impression is of the same quality of black throughout. The question now is: Can we combine the gradated biting through a film exposed under a half-tone positive with the ink-holding capacity of a plate treated as just described? The desired result can be accomplished by first exposing the plate coated with bichromated gelatine under the screen, then exposing it a second time under the half-tone positive and then biting it in (etching it). The screen secures the grain or tooth needed, and the half-tone positive secures the gradations in depth between the ridges of metal which form the grain; and as the mordant bites not only downward but also sideways, it reduces at the same time the size of the metal ridges in the darkest parts, while it leaves them broader in the lighter parts, the action varying according to the energy which the mordant exercises on the various parts of the plate. It will be seen, therefore, that the gradations produced are the result not only of the depth of

the ink-holding cavities but also of the quantity of metal left standing between them. This is the process called *photoglyptic engraving* by Fox Talbot, and patented by him in England, in 1852. In the same patent nearly all the other contrivances are described which have since been perfected, and which have given their usefulness to the photogravure processes of to-day, most of which are based on them. Other experimenters have also used screens in photogravure processes — among them Egloffstein, one of the earliest workers, if not the earliest worker, in this field in the United States; but it may be said that they have been practically abandoned, and that an aquatint ground laid on the plate itself has been substituted for them. Fox Talbot was again the first to suggest and use such a ground, but the method of forming the film in the photo-aquatint process of to-day is the result of developments which have taken place since Talbot's day.

The *photo-aquatint process*, the perfecting of which is generally credited to Klic, of Vienna, and by which the very largest part of the photogravures published at present are made, involves the pigment or carbon printing process, and we must therefore familiarize ourselves with this latter before we can proceed. The pigment printing process is an outcome of the desire to substitute a material promising more stability for the silver which forms the image in the "silver-prints" of ordinary photography. The aim was to substitute carbon, which is the coloring matter of printing ink, or other stable pigments for the silver; but as these bodies are not affected, or at least not sufficiently affected, by light, their direct use was out of the question. It was seen, therefore, that they must be combined with some other matter which had the required qualities, and which might be removed after exposure in proportion to the action of light on the various parts of the picture. Of course the removal of this medium would carry with it, also, the carbon or other pigment, and, as more or less of the medium was left, more or less of the carbon or pigment would likewise be left; and thus a picture in a permanent color — as far as permanency goes in mundane things — would result, its permanency depending, however, on the permanency of the medium. In the history of this process we have again a subject which might easily tempt us to linger over it, for it is exceedingly curious to see in it how long it generally takes mankind to find out what seems patent to every one after it has been found out. However, we must check ourselves, and be content with an outline of the process as it is practiced to-day.

The first step in the *carbon or pigment printing process* is to coat sheets of paper with gelatine mixed with carbon or some other inert pigment. In this condition the sheets may be kept for a long time. If a carbon print is to be made a sheet of this paper is soaked in a solution of potassium bichromate, and dried in the dark. It is then exposed to the light under a negative, say a negative made directly from nature. Upon the removal of the negative after the exposure the surface of the paper — of the "carbon tissue," as it is technically called — is still simply black, blue or of whatever color the pigment mixed with the gelatine may have been. The light has penetrated most deeply where its action was strongest, that is to say, under the clear parts of the negative, and has hardened the gelatine as far as it has penetrated. In the other parts it has penetrated more or less deeply according to the varying degrees of density in the negative, and has, therefore, also hardened the gelatine film to a greater or less depth. Under the densest parts of the negative it has left it entirely soluble, or at least very nearly so. Now I want you, if you please, to stop once more to do a little extra thinking and a little extra admiring. What is the result of the operations I have just described to you? The result is that we have imbedded in our film, which is so thin that we cannot measure it by ordinary means, a relief in hardened gelatine which in its variations of height represents all the innumerable shades and gradations of the original! The delicacy of such a relief is so extraordinary that we cannot realize it, and yet our reason tells us that it must exist when we finally see the result on white paper. Here again is one of those marvelous facts, one of those great wonders to which we are indifferent simply because we do not stop to think. The question now is how to get at this relief; how to take it out of the surrounding mass of gelatine which has remained soft. The answer seems simple enough: Wash it out! But here an unpleasant difficulty presents itself. The sheet on which the film is spread was, of course, exposed under the negative on the film side. The action, therefore, began on this side, and it follows that the basis of the relief, that is to say, its back or flat side, is on top, while the soft gelatine which is to be washed away is between the relief and the sheet of paper which bears the film. If, therefore, the sheet were washed in the condition it is now in our hardened gelatine relief would float away in pieces, and our labor would be lost. To prevent this disaster it is necessary to attach the sheet to a second sheet, film

downwards, by means which need not here be particularized, before the "development" or washing away of the soft gelatine takes place. This done, the relief has a support on its under or flat side, and consequently cannot float away. You will now understand from what I have told you that a carbon or pigment print is a film of hardened gelatine charged with a pigment, varying in thickness according to the depth of shade to be produced, and attached to a sheet of paper or some other substance, as, for instance, glass, in which case the resulting picture is called a carbon transparency. The process here described — the single transfer process — is available only in case a reversed negative has been used, or for subjects in which the question of right and left is of no importance. With an ordinary negative the double transfer process must be resorted to, that is to say, the development must be done on a temporary support, and the developed picture transferred once more to its final support.

We may now proceed with the examination of the *photo-aquatint process*, as it is generally practiced to-day. The first step will be to lay a dry aquatint ground on a metal plate. This is done by powdering the plate with some resinous substance, usually asphaltum, and then heating it just sufficient to make the particles adhere to it. The minute specks of resin thus fastened to the plate being indifferent to the mordant protect the plate from its action, whereas in the channels between them it goes on without hindrance. The aquatint ground, therefore, serves the purpose of the first exposure under a screen, as practiced in Fox Talbot's photoglyptic process. Over this ground is mounted a negative gelatine film made by the pigment printing process. To obtain this a reversed positive on glass (carbon transparency) has first to be made, the necessity for which will become apparent when the nature of the manipulations in the pigment printing process, which involve the turning of the film, are considered.

We have now a metal plate, generally of copper, with an aquatint ground laid on it to produce the tooth or grain, and mounted over that a hardened half-tone negative gelatine film, thickest where the biting is to have no, or the least, effect, and gradually becoming thinner in those parts in which it is to increase in depth, and therefore fit to regulate the biting in accordance with the gradations to be obtained. By way of parenthesis I may add here that the pigment in the gelatine film is not an absolute necessity. A clear gelatine film would do as well; but it is obvious that the pigment facilitates the operation of developing

the film, inasmuch as it enables the operator to see more clearly what he is doing. The rest of the process requires no further explanation, so far as the principles involved are concerned, in the light of what I have already told you. A few words must be said, however, concerning the mordant used, and the manner of using it. Two kinds of mordants are employed for etching — effervescent, such as nitric acid, which evolve gas, and therefore bubble, and still, which act without effervescence. An effervescent mordant cannot be used in the processes which make use of a gelatine film as a resist, since the biting takes place *under* the film, and the bubbles, which rise with great energy, would consequently tear it up. A solution of perchloride of iron is therefore used, which is a still mordant. It is worth noting, also, that successive baths of varying strength are used for the biting. A concentrated solution of the perchloride penetrates only the thinner parts of the film, while a more diluted solution acts also through the thicker parts. The biting, therefore, is begun with a strong solution, which acts only in the darkest parts, and this is followed up with weaker and weaker solutions, which continue the biting in the darks, and at the same time carry it on gradually towards the lights. You will see from this that the gradations do not depend simply on the variations of permeability in the film, but that they are assisted by the succession of solutions of different density which are under the control of the operator. The biting having been completed the plate is worked over, if necessary, with the burnisher, to brighten the lights, and with roulettes, gravers, etc., to strengthen the darks

A modification of the photo-aquatint process, into which the pigment printing process does not enter, is known as *photogravure Gilbo.* A bichromated gelatine film is formed on the metal plate, and exposed under a positive. After exposure an aquatint ground is laid on top of the film and fixed by heat, as usual. The mordant is again perchloride of iron (or nitrate of silver); but instead of beginning with a concentrated solution, a weak one is first used, so as to produce the lighter shades upon the plate. The film is then removed and a proof taken. A second film is now laid on the plate, with an aquatint ground, and the biting is repeated. The second aquatint ground used is coarser, however, than the first, and the biting is done with a stronger solution, so as to leave the delicate tints as they were obtained by the first biting, while the depth of the middle tints is increased. The operation is repeated a third, and even a fourth time, etc., if necessary, with in-

creasingly coarser grounds and stronger solutions, so as to give the final strengthening to the deeper shades of gray and to the blacks. The plate is completed by burnishing, rouletting, etc., as before.

There are other photogravure methods, but the tendency is to keep them secret. One of the processes, for instance, worked by Boussod, Valadon & Co. (formerly Goupil & Co.), of Paris, and invented by their technical manager, Mr. Rousselon, is understood to be a deposit process, the printing plate being produced by means of electro-deposition on a wash-out relief — a Woodbury relief, of which we shall hear more presently — charged with gritty matter. The beauty of the plates produced by this process is largely due, however, to the careful and artistic hand-finishing bestowed on them.

Our next subject will be the *planographic half-tone processes*, or, in other words, half-tone photolithography and zincography. Again we are tempted to linger over the history of the subject, and again we must confine ourselves to a furtive glance — a necessity all the more to be regretted as the history of photolithography is quite varied. In the older processes, notably that of Lemercier, Lerebours, Barreswill and Davanne, asphaltum was used directly on stone; Poitevin essayed albumen and bichromate directly on stone; Asser worked out a transfer process with starch and bichromate, and Toovey used gum and a bichromate for the same purpose.[1] All these processes have come to naught, however; and so has that, also, of Lodovic H. Bradford, which, nevertheless, I shall ask you to consider a moment on account of the local interest which it has for us, since Bradford was not only one of the early photolithographers of America, but moreover a Bostonian. In his process a lithographic stone, grained for half-tone or polished for line work, was sensitized with gum arabic and a little sugar, mixed with potassium bichromate, and exposed under a positive or in the camera. After the exposure the unchanged gum was washed away, leaving the stone exposed in those parts which were to take the ink. It was then treated with soap water, rolled up in ink and etched like an ordinary lithograph, the etching removing at the same time the hardened gum on the exposed parts. Bradford's process was patented in 1858 (by Cutting & Bradford). Specimens of it still extant — portraits and portrait groups made under positives, and views in Boston photographed directly on the stone in the camera — show that, like most similar processes, it gave results which came

[1] For summary accounts of these processes, see the catalogue before referred to.

very near success and yet never reached it fully. Very likely it failed, in company with all other direct processes, from the fact that the development had to be done from the wrong side, that is to say, from the surface which had been exposed to the light. The unchanged matter to be washed away was, therefore, under the hardened surface, and the result was a somewhat ragged image lacking in half-tones. Some of these processes, moreover, were not at all true photolithographic processes — a term that can be applied to such processes only in which the image is really formed by combination of fatty acids with the stone or zinc, as in the original planographic processes. In Lemercier's asphaltum process, as well as in Poitevin's albumen process, the ink was taken up, not by the stone itself, but by the hardened asphaltum and albumen left on the stone after development. As this wore away, the ink, no doubt, gradually penetrated to the stone; but this was an uncertain process, and in it, probably, lies the explanation of the fact that stones prepared by these methods yielded too few impressions to make them commercially available. Nevertheless, I must not neglect to tell you that the asphaltum process directly on stone under negatives has lately come into use again somewhat extensively for color work, rather as an auxiliary to the lithographic artist, however, than independently.

The processes mostly used to-day to produce half-tone on stone or zinc are transfer processes, based upon the reticulation of gelatine. These transfers are made from gelatine films exposed under negatives, and so treated, by the admixture of chemicals and drying under special conditions, that they assume a reticulation or grain varying in size and closeness according to the amount of light that has acted upon the different parts of the film. If such a film is inked while wet it takes up the ink on the exposed parts, as I told you before, in proportion to the exposure. An impression from a film so rolled up is made on transfer paper, and then transferred to stone or zinc in the usual manner. These films are really collographic printing surfaces — of which we shall hear more shortly — with a grain purposely exaggerated. Photolithographs of this kind may be found among the illustrations of the *American Architect*, and the "ink photos" made by Sprague & Co., of London, are the result of a similar process.

A late form of half-tone photolithography, called the *Bartostype*, from its inventor, J. Bartos, of Bohemia, is based on the capacity of

hardened bichromated gelatine to withstand the impact of the sand blast in proportion to the length of exposure. A stone or zinc plate is coated with a varnish made of asphaltum and mastic. Upon the stone or plate so prepared a gelatine wash-out relief, made by the pigment printing process, is mounted and treated with a mixture of glycerine and water, in which a small quantity of alum has been dissolved. The stone or plate is now exposed to the sand blast, which destroys the gelatine film or relief, at first in its thinnest parts and gradually also in its thicker parts. The destruction of the gelatine film lays bare the varnish, and allows the sand blast to act on it in proportion to the gradations of the original from which the relief film was made. The result is a picture on the stone or plate in which the darks are represented by the varnish, the lights by the bare stone from which the varnish has been removed by the sand blast, and the gradations between the two extremes by the varnish more or less perforated by the blast. The stone or plate is now gummed, and after the varnish has been removed with turpentine it is rolled up, and otherwise treated like a lithographic transfer. The result, as you will see, is a true lithograph.

We now come to what is, technically considered, the greatest triumph of the photo-mechanical processes, *the production of relief-blocks from half-tone originals*, — the transformation of a portrait photograph, or of a photograph from natural scenery, or from a washed drawing or a painting, into a block broken up into masses of dots, according to the requirements of relief printing, so that it can be worked in the type press, — and all this without the intervention of designer or engraver.

Once more we must ignore history, and even such interesting early attempts as Paul Pretsch's,[1] which involved the reticulation of gelatine; but once more, also, we must make an exception in favor of at least one extinct process, partly owing to the peculiar interest it offers, and partly again from local considerations, although the inventor in this case was not a Bostonian, but a Philadelphian. I allude to the original *Ives process*, invented by F. E. Ives (1881), and not to be confounded with the process at present called by the same name, which is merely an application of the screen process to be described hereafter. The Ives process may be outlined as follows: If a bichro-

[1] See the catalogue before referred to.

mated gelatine film, spread on a glass plate, is exposed under a half-tone negative, and after exposure is washed, so as to remove the unchanged bichromate and thus stop further action under the influence of light, and at the same time to cause those parts of the gelatine which have retained the capacity of absorbing water to swell up, we shall have a relief, the highest parts of which represent the high lights of the original, while, *per contra,* the lowest parts represent the blacks. It follows, furthermore, that the various grays will be represented by elevations in the relief varying with the value of the grays. Such a swell-gelatine relief forms the starting point of the Ives process. From it is made a plaster cast, in which, as a matter of course, all the relations are reversed, that is to say, in which the blacks are represented by the highest, the whites by the lowest parts. Against this cast is pressed a gelatine film, mounted on a glass plate, which has been cut up into lines, or into dots by lines crossing one another, and charged on the surface of the lines or dots with printing ink. As this film comes into contact with the cast, the lines or dots, being elastic, are compressed in proportion to the height of the relief, and therefore produce on it larger and smaller black dots; while in the deepest parts, where the lines or dots cannot reach the surface of the cast, it remains white. The result is that the relief is covered with black dots, which are largest in those parts of it corresponding to the blacks of the design, and gradually decrease in size as the gradations of the original approach white. From the cast so stippled a black and white negative is made, and by its means a block, by either the etching, swell-gelatine or wash-out method.

A process similar to the Ives process was invented by Petit in France. He blackened a plaster cast from a swell-gelatine relief all over, and cut through its surface in parallel or crossing lines by a V shaped point, traveling always in the same plane. The point cut a wider line in the highest parts of the blackened relief, and therefore removed more of the black in them than in the lower parts, while in the very lowest, which it could not reach, it left the black solid. It is evident from this that Petit must have made his relief from a positive, or else must have made a true cast from the mold first obtained from the relief.

Another method for obtaining half-tone relief blocks is the *mezzotype* or photo-aquatint in relief. Briefly stated, it is the reversal of the photo-aquatint intaglio process. A sensitized, that is to say, bi-

chromated gelatine film is formed on a metal plate, and an aquatint ground is laid on it after it has been exposed under a reversed negative, or a positive gelatine film produced by the pigment printing process is mounted over an aquatint ground, and the whites are bitten away by a still mordant.

Most half-tone relief blocks, however, are at present made by what may be called the *relief screen process.* The most interesting and important part of these processes is the making of the black and white half-tone negative, for, this negative once produced, the operations that follow are immaterial to us, as the block may be made from it by any suitable means.

Now, I said purposely the *black and white half-tone negative*, a form of expression which seems to involve, and under ordinary circumstances would involve, a contradiction in terms. For what is "black and white," in the sense in which we have used this expression, cannot at the same time be "half-tone," that is to say, cannot show gradations from black to white through "structureless grays." Nor do the relief half-tone blocks show structureless grays; they merely simulate them by breaking up the structureless masses into masses of black dots interspersed with more or less white, and they try to make these dots so small and to place them so closely together that even at a short distance they will merge into grays on the retina. They try, in fact, to be among relief printing processes what mezzotint is among the intaglio processes, for we have seen that even in the most delicate mezzotint the grays are not really structureless. But, as a matter of course, they will never be able to rival mezzotint in fineness and richness, owing to the limitations of relief printing.

As before stated, the main interest in the screen processes is offered by the making of the black and white half-tone negative, and we must, therefore, above all things, make clear to ourselves the difference between such a negative and an ordinary half-tone negative. It will be best understood if the latter is considered as a washed India ink drawing, and the former as a stipple drawing made from it with the pen, with absolutely black ink and with mathematical precision in the placing of the dots; and in which, moreover, the dots in the darker portions are so managed as to run together in such wise that the appearance is produced of white dots on a black ground — these white dots growing smaller and smaller until they disappear altogether

in the solid blacks. The question now is: How can this transformation be accomplished, or, more correctly speaking, how can such a negative be obtained from an original of any kind? The process is simple enough, at least apparently. A wet collodion plate is placed into the camera, and before it, at a slight distance from it, a glass screen ruled with fine black lines, — in most cases, or perhaps in all cases at present, lines crossing one another. The fineness of these screens may be realized from the fact that the number of lines to the inch ranges from eighty to two hundred and forty. The rays of light reflected by the picture or other object to be photographed naturally pass through the clear spaces of this screen on their way to the sensitive plate, while all light is cut off by the black lines of the screen. It is evident that the image produced on the plate must consist of isolated dots. But what kind of dots?

We are here brought to another thinking stop by a most curious action of light, which the facts so far stated do not in the least explain. Nor will the limits of time set me allow me to enter into details concerning the explanations hitherto attempted with — as it seems to me — but little success. I must, however, call your attention to the difficulties in the way.

It is, of course, an easy matter to make from a positive a negative, or from a negative a positive, broken up into isolated dots. We may draw lines on the original crossing one another, or we may perforate it, or we may lay some kind of netting upon it, and having so prepared it, we may make our negative or positive from it, as the case may be. The result, however, will be a picture in *dots of equal size but differing in intensity*, that is to say, some black, some dark gray, some lighter gray and so on. Here is a photograph of this kind made by Mr. Thomas Gaffield quite a number of years ago, before any one, at least in this country, was thinking of half-tone relief blocks. It is distinctly *broken up into dots*, but they are *equal in size and different only in intensity*. Now we know, from our previous investigations, that a negative of this kind will not do for the making of relief blocks, since gray lines or dots will either not reproduce at all, or will be rotten, or will come up a solid black, and will, therefore, be too heavy. It was this experience, as we saw, that led to special methods of drawing for process work. What is wanted is a negative having dots not of the *same size and differing in intensity*, but on the contrary *differing in size and of the same intensity*. And this the screen, as used in the

process here illustrated, gives us. Where the brightest light falls upon the sensitive plate in the camera through the apertures in the screen it overpowers to a certain extent the black lines of the screen, the action diminishing as the force of the light diminishes, until in the blacks there is no action at all. Or it may be said that centers of development are first established where the brightest light penetrates, the action again diminishing, or beginning more tardily, as the intensity of the light reflected by the original diminishes. However this may be, the result is that the succeeding development and intensification of the latent image produces equally black dots, differing in size, and therefore, as a matter of course, interspersed with more or less white (since the distance from center to center of each dot is always the same), in tolerably exact proportion to the gradations of the original.

The use made of the screen in these relief processes must not be confounded, as has sometimes been done, with the use made of it by Talbot in his photoglyptic (intaglio) process. A little consideration will show that the principle involved, as well as the result reached, is quite different. The oldest known process belonging to the group under consideration seems to be that patented by Moritz and Max Jaffé, of Vienna, on March 1, 1877, in which bolting cloth was used as a screen. The practical introduction of the screen processes for relief work dates, however, from the year 1882, when Meisenbach, of Munich, took out his patent. The descriptions given of the manner in which the screen is used in the Meisenbach process vary, and it is quite likely that changes have been introduced since it was first made known. The present method seems to be to place a screen ruled diagonally (not cross-lined) at a slight distance before the sensitized plate in the camera, and after a short exposure to turn the screen, or rather to substitute another for it, so that the lines on it run in the opposite direction, and to expose the plate a second time.

The unpleasant features of these screen processes are: — The uniformity of texture produced by them; the mechanical regularity of the grain, to overcome which screens of other patterns, with irregular grains, etc., have been tried; and the impossibility of rendering white, since the screen, even in the highest lights, asserts itself sufficiently to produce a light gray. This last difficulty may, indeed, be overcome by a judicious use of the graver or by the skill of the etcher; but unless these adjuncts to the process are employed artistically, which they rarely are, they are more likely to mar than to help the beauty

of the effect. Nevertheless it must be admitted that, with suitable originals and with the best of printing, these half-tone relief processes have given results which are very attractive in their delicacy.

The question as to the way in which the light acts in these processes is complicated still further by the various modifications which have been introduced for the attainment of the same result. Negatives have been sprinkled, in which case the screen may be said to lie directly on the film; glass plates have been prepared with a sensitive film, exposed under a screen, developed, and a second sensitive film laid on the plate, so that the screen lies under it; and barium sulphate has been mixed with the sensitizing mixture, forming opaque specks in it, in which case, manifestly, the screen is in the sensitive plate itself. Finally a process was patented not long ago, in which the picture is projected on to a lined screen, and a half-tone negative obtained by photographing the image on the screen. All these processes, however, seem to give but imperfect results, and I believe I am warranted in saying that none of them are practically in use.

We must finally devote such time as is left us to two processes which are of quite peculiar interest, as not only the methods used in the production of the printing forms, but even the resulting printing forms themselves, are entirely novel. The plates and blocks produced by the processes so far investigated differ essentially, in their technical features, in nothing from the plates and blocks produced by the old hand processes. The *photo-relief blocks* are inked and printed just like an engraved wood block or an electrotype from it; a *photo-intaglio plate* is treated just like any other intaglio plate; and a *photo-lithograph* or photo-zincograph, once produced, differs in nothing in its treatment from an ordinary lithograph or zincograph. It is different with the *collographic process* and the *Woodburytype*. They come as near as may be to the requirements involved in the term "a printed photograph," and indeed most people find it difficult to distinguish them from ordinary photographs. Furthermore, the Woodburytype has the unique distinction of being that seeming impossibility,—a picture produced in a press, and yet, without the least doubt, offering *structureless grays*.

The *collographic process* (heliotype, collotype, phototype, albertype, artotype, etc.), from *kolla*, glue, and *graphein*, to grave, is so called because the printing form is a film of gelatine, which is glue, on a support of some kind. Of these supports a great many have been tried,

with a view to securing a firm attachment of the film. The great difficulty with the earlier operators; among whom Tessié du Mottay and Maréchal may be named as the more successful, was to prevent the film from tearing away from its support. The consequence of this tearing away was that only a small number of copies could be printed at a time. Of these various forms of the process we can, of course, consider only a very few. The general feature of all of them is that films of bichromated gelatine, to which a small quantity of chrome alum has been added, are dried under certain conditions of atmosphere and temperature, which are secured by the use of specially constructed drying ovens, and are then exposed under reversed negatives. These latter are by preference, in this country at least, film negatives, that is to say, instead of being mounted on glass they are mounted on gelatine films. This is due in the first place to the ease of preservation offered by such negatives, their preservation being necessary, since, even with all modern improvements, a collographic printing film is still a tender object, and may, therefore, have to be renewed repeatedly, especially in case of a long edition; and, secondly, to the fact that they give better contact. After exposure the film is washed, so as to remove the unaltered bichromate, as otherwise the gelatine would harden uniformly throughout under the influence of the light during the process of printing. A film so treated consists of gelatine in its normal condition in those parts representing the lights of the picture, of hardened gelatine in those representing the blacks, and of gelatine hardened proportionately more or less in the gradations between white and black. It is, in fact, a swell-gelatine relief, in which, however, the relief is purposely kept as low as possible so as not to interfere with the inking. If such a film is rolled up in printing ink while it is moist, it accepts the ink in due proportion on the blacks and grays of the picture, and rejects it on the whites, like a lithographic stone. The printing is, indeed, done on lithographic presses (except in one of the varieties to be mentioned) slightly modified, that is to say, with less tension. To keep the film moist and yet guard it from injury by repeated manipulation, it is soaked — or "etched," as it is improperly called — in a mixture of water and glycerine, to which a small quantity of ordinary salt has been added. The inking is usually performed with two rollers,— a leather roller with stiff ink for the darker parts of the picture, and a glue roller with thin ink for the lighter parts and tints. Steam presses are also largely used for collographic printing.

The most common supports used for the printing films are glass plates. Albert, of Munich, who coined for his products the name of "Albertotype" or "Albertype," is said to have been the first to overcome successfully the difficu'ty of making the film adhere to the glass. He formed on the latter a preparatory film of albumen and bichromate, which he exposed through the glass, so that its hardened side was in direct contact with the glass. The second, or printing film, was then formed on this preparatory film.

An improvement on Albert's method, which did away with the necessity of two exposures, was the substitution for the bichromated albumen of water-glass and albumen, or water-glass and stale beer, for the preparatory film. Water-glass, which is a soluble alkaline silicate, forms an insoluble porous body with organic substances, such as albumen, etc., and adheres to the glass with great tenacity, thus forming an excellent substratum for the printing film.

Copper plates slightly roughened are also used as supports, and in this case no preparatory film is needed as the film adheres sufficiently to the metal. Their use is more limited, however, than that of glass plates, in consequence, probably, of higher cost and of their opacity, which does not admit of the examination of the film from the back during the process of exposure under the negative.

Another variety which I may mention to you is that in which loose printing films of gelatine toughened by the admixture of alum are used, the moistened film being attached to temporary metal supports by atmospheric pressure, and the printing done in platten presses (with pressure from above) instead of lithographic presses (with scraping pressure). The name "heliotype" has been more especially applied to this variety, which was invented by Ernest Edwards, formerly of London, later of Boston, and now of New York.

After this mere statement of facts we are again confronted by the question: How is the action of the light to be explained in this case? And this question will have to be followed by another: Why does the bichromated gelatine act as it does after exposure? The simplest way is to assume that the light closes the pores of the gelatine in proportion to the amount of exposure, and consequently robs it in inverse porportion of its faculty of absorbing water, so that it accepts most ink where it is driest, less where it is more moist, and none at all where it is charged with water to its full capacity. But it is apparent, upon examination of a collographic print or plate, that it

shows a decided grain, due to the vermiculation or reticulation of the gelatine, however fine that may be. Husnik describes a plate promising good results as follows: "It is exceedingly fine grained, almost transparent in the highest lights, increases in roughness as it approaches the half shades and shades, and again decreases somewhat in the darker shades, until sometimes, in the absolute blacks, no grain at all is visible." Nevertheless, the same authority maintains that plates without any grain,—which can be obtained by a change in the method of drying the film,— can also be used for printing, provided they can be kept moist, which is more difficult than with grained plates, as these open a passage to the water through the clefts between the vermiculations. We may take leave of the subject with the remark of Husnik, that "it is difficult to explain the formation of the grain," without following him into the explanations which he suggests.

We have now reached the last chapter in our review of this vast and interesting field, to wit, the *Woodburytype*, so named from its inventor, and, as I have already told you, the only printing process that can claim to produce structureless grays. Gelatine is the main factor in this process, to a greater extent even than in the other processes which we have investigated. A bichromated gelatine film is formed on a piece of glass coated with collodion. After it has been dried in the dark and stripped from the glass it is exposed from the back, that is to say, the collodion side, under a straight (unreversed) negative. It is then mounted on a glass plate with rubber cement, collodion side down, and developed like a carbon print. To facilitate the development by enabling the operator to watch its progress, the gelatine has been charged with carbon, and the result is, therefore, a true carbon print, or in other words one of those marvelously delicate reliefs in hardened gelatine which I asked you before to admire when we were speaking of the carbon or pigment printing process. This relief having been dried and hardened, it is stripped once more, laid, face downward, on a plate of lead, and exposed to powerful pressure. Our first idea will be that the delicate relief will be crushed and the lead plate left flat as before. This is not the case, however. The gelatine relief is left perfectly intact, so that it can be used for the same operation a number of times, and the lead plate has been transformed into an exceedingly shallow mold, deepest where the relief was highest, and highest where the relief was lowest. This lead mold, therefore,

is in the same condition in which a metal plate would be which had been etched through a half-tone gelatine film, without some devise for producing a grain. Naturally, if the mold were filled with printing ink and wiped as usual, the result would be a failure. The difficulty is overcome in this case by the printing ink and the press used. The printing ink is a hot solution of gelatine, in which a pigment is suspended; the press is virtually a platten press, in which the pressure is exercised by a flat platten from above, which presses the paper against the form, but not into its cavities. The form is oiled, the hot gelatine solution poured into it, paper of a special kind laid upon it, the platten brought down, which squeezes out the excess of ink, and the whole is allowed to rest until the gelatine has cooled and set. The press is then opened, and the paper carefully removed. It goes without saying that the gelatine ink adheres to the paper, and is lifted by it out of the oiled form. The result is a gelatine relief of the utmost delicacy attached to paper, — in reality a carbon or pigment print produced mechanically. The process has given most admirable results, unrivaled in brilliancy and depth of effect, as well as in sharpness of detail; but it is, nevertheless, open to objections which have prevented its being used to the extent which it would otherwise deserve. In the first place, as it is necessary that each impression should "set" before it can be removed from the press, heat interferes with the printing. A second disadvantage is the fact that each impression must be mounted like a photograph. But the greatest difficulty arises out of the nature of the gelatine relief which forms the picture. Not only are the darks perceptibly higher than the lights, which is in itself in many cases a disadvantage, but the gelatine in which the carbon or pigment is held is apt to become brittle and to chip off with age, despite the hardening processes with alum, etc., which it is made to undergo. In this respect the Woodburytype differs from an ordinary carbon print, which otherwise it so much resembles. In both cases the medium for the pigment is, indeed, gelatine; but in the case of the carbon print it is gelatine hardened and changed throughout its mass by exposure to light in the presence of a bichromate, while in that of the Woodburytype it is gelatine, but indifferently affected by subsequent treatment. This is the reason, also, why the Woodburytype has never taken a foothold in our own country, with its changeable climate. One manufacturer there was who tried persistently to conquer the difficulties in the way, but if you should visit him to-day he would

point to a few specimens nailed up like dead bats against his office door, saying at the same time: "That is all there is left of the fifty thousand dollars which I put into the Woodburytype process." And yet some of these specimens, made about twenty years ago, are as good to-day as they were then. It is evident, therefore, that under certain conditions these prints may be made to stand, even in America, if only those conditions can be found out.

Before I take leave of you I must warn you once more that the statements made in these lectures are of the simplest and most superficial kind. From the descriptions I have given you it might be inferred that the photo-mechanical processes are easily worked. Nothing would be further from the truth, however. They are beset with innumerable difficulties, of which temperature, hygrometric conditions, variations in exposure according to light, quality of negative, etc., and the tricks of gelatine, which is a most variable and unreliable substance, are but a few. "Gray, my dear friend, is all our theory, and green the golden tree of life," says Mephistopheles to the student whom he would mislead; but in this case — and perhaps in some others — theorizing is decidedly easier and more pleasant than practicing, and the tree of life is beset with thorns.

And this leads me to my final remark. So difficult a subject, and no help extended to the student anywhere, at least in this country! The literature is a chaos, the "practical" men work by rule of thumb, shun theory, strive to keep their little tricks secret, and some of them, alas, would not scruple to mislead the anxious inquirer who turns to them for information. In Europe, at least in Germany and Austria, there are several government schools in which not only pupils are taught, but a chance is given for purely scientific investigations. That such investigations are needed must have become apparent to you more than once in the course of my remarks. And that the "practical" man might also be helped by having his operations placed upon a sure foundation of knowledge is a self-evident fact. Shall I add, in the spirit which measures everything by money as the most important factor of all, that large interests are involved in this industry — interests that deserve as much attention as others? However all this may be, it is a curious fact that not a single school exists to-day in the United States to which a young man might turn for thorough training in these branches, and not a single laboratory to which the operator might go for aid in the difficulties which beset him. Would

it not be worth while for a great seat of practical science, like the Massachusetts Institute of Technology, to give some attention to these important matters?

The above lectures were fully illustrated by materials, tools, and plates, as well as by the finished products resulting from all the processes mentioned, and in connection with them an exhibition was held at the Museum of Fine Arts, from January 8 to March 6, 1892, comprising 734 specimens, arranged chronologically. Of this exhibition a catalogue was published with technical and historical explanations, under the following title: "Exhibition illustrating the technical methods of the reproductive arts from the fifteenth century to the present time, with special reference to the photo-mechanical processes. January 8 to March 6, 1892. Boston: 1892." 12mo., xi and 98 pp.

MODERN SCIENTIFIC VIEWS OF THE CAUSE AND PREVENTION OF ASIATIC CHOLERA.

By WILLIAM T. SEDGWICK, Ph.D.

Read October 6, 1892.

The first requisite for intelligent action is precise knowledge. Before we can deal wisely with a disease we must know what it is and how it acts. It is for these reasons that we must inquire, What, precisely, is Asiatic cholera, and how is it disseminated? Then, and then only, shall we be prepared to consider the means to be adopted for its prevention or suppression.

Asiatic cholera, according to the best knowledge of to-day, is a very active fermentation of the intestinal contents of man by a specific living ferment known as the *Spirillum* or *Bacillus* of Asiatic cholera. This fermentation differs from all other known fermentations of the intestinal contents in its activity and in its peculiarly destructive effects upon the individual. From the biological point of view it closely resembles the fermentation which occurs in typhoid fever, but it differs from that of typhoid fever very much as an explosion of dynamite differs from an explosion of gunpowder: it is more rapid, more violent, more fatal. The fermentation of the contents of the alimentary canal by a microörganism might be accompanied — as in the digestive processes of every-day life it unquestionably often is accompanied — by the formation of products harmless and even useful to the animal economy. But it appears to be the distinctive characteristic of the specific organism of Asiatic cholera that the fermentation to which it gives rise is accompanied by the development of products peculiarly poisonous to the human body — poisons which, absorbed into the body, upset the nervous system, producing vomiting, cramps and collapse; and in a very large proportion of cases produce death.

To make it clear what a disease of this kind really is the analogy is often used, and is just, of the fermentation of apple juice by the microörganism known as yeast. Under the influence of this specific

ferment innocuous apple juice is changed, so that it contains a small quantity of a poisonous substance, alcohol. If it contained a great quantity of alcohol, or if alcohol were a much more poisonous substance than it is, fermented apple juice would act upon the body as a violent poison.

So far as we know at present cholera cannot be produced without its specific ferment or germ, precisely as cider cannot be made without yeast or oaks spring up except from acorns. No matter how fertile the soil may be, the seed must be sown before the crop can appear. To suppose that cholera can arise without its germ is to assume the truth of spontaneous generation — one of the out-worn creeds of scientific history.

It is not necessary for our present purposes to consider at length the particular character of the organism of cholera. It is now generally believed that the curved bacillus or spirillum discovered by Koch, and known as the "comma bacillus," is the one and only cause of Asiatic cholera. But whether this be the case or not, there can be no question that cholera is a fermentative disease produced by a specific living microörganism, and for the purposes of practical sanitation this is sufficient. To enter upon an elaborate description of the organism or a history of cholera in the Middle Ages would be interesting, but it could not instruct us in modern scientific sanitation, which for its basis rests upon discoveries made since our civil war. To follow cholera in its rôle as a haunting spectre of unclean Orientals on filthy pilgrimages, is certainly impressive and not without its lesson; but the man-eating tiger is not to be feared in America as he is in India, and we may as reasonably hope to control the cholera from Calcutta as the tiger from Bengal.

If one wishes to have a fairly accurate idea of the germs of Asiatic cholera he has only to recall the chains of sausages which hang in butchers' windows. Enormously magnified and seen at a little distance the chain, with its curved links of sausages, well represents the growing groups of comma spirilla. Each individual sausage, moreover, well represents one germ or spirillum. Like the sausage the spirillum has a delicate skin or cell wall. Like the sausage the spirillum contains a substance of unknown composition. But unlike the sausage the spirillum is a living organism, and is able to reproduce itself.

We may now inquire how, precisely, is cholera disseminated. If what has been said above is true it is obvious that the principal and

prolific source of cholera seed must be the contents of the alimentary canal; and exactly as a little leaven may leaven the whole lump the contents of one intestine may theoretically suffice to sow a thousand crops. Whether a single germ can or can not convey the disease we do not know. The probability is that under favorable conditions it might do so, but it is doubtful if, as a fact, it ever does. But in any case the seed must enter the body; and inasmuch as the body is virtually hermetically sealed in a germ-tight skin the germ must enter the mouth. Even supposing the skin to be broken it is at least doubtful if the germs could find effective entrance along that road. The only important avenue of entrance is unquestionably the mouth — using the word in its embryological sense and including the nose as a part of the mouth and as an avenue for dust. Whether the germs of cholera thrive or do not thrive outside of the human body they must still find entrance to the alimentary canal chiefly, and probably exclusively, through the mouth. We have then simply to consider in what ways the germs of cholera may be carried to the mouth.

Here we may get much help from a careful study of the facts of infection in a closely related disease with which we are much more familiar. I refer to typhoid fever; and, as I propose to draw largely upon our established knowledge of typhoid fever to offset our ignorant and foolish alarm about cholera, I may be permitted to say that while, in common with most Americans of my age, I have had no personal experience with epidemics of cholera I have had, within the past two years, a considerable experience with epidemics of typhoid fever, having superintended for the State Board of Health and others the investigation of seven important outbreaks of this disease in Massachusetts.

Typhoid fever, like Asiatic cholera, is a diarrhœal disease; like cholera it affects the alimentary contents by a peculiar fermentation; but it is to cholera as gunpowder is to dynamite — slower, less violent, less fatal. It is almost absolutely certain that both diseases — typhoid fever and Asiatic cholera — are disseminated chiefly through the fæcal discharges. We have already seen that the germs of cholera must enter the body through the mouth; the same is true of typhoid fever. In both diseases, therefore, the problem of dissemination is reduced to this: How may the discharges from the bowels of patients affected with cholera or typhoid fever be conveyed to the mouths of other people; or, reducing the problem to its lowest terms, How may the

bowel discharges of A reach the mouth of B. The answer to this question should give us, and in fact does give us, the basis of a rational procedure in prevention.

We may stop at this point long enough to observe that if what has been said is true — and I have no hesitation in affirming most emphatically that it is — the recent extraordinary exhibition of popular alarm in this country was most discreditable. If there was any reason for it, that reason must have been a distrust of our capacity to keep bowel discharges out of the mouths of the people. If there was no reason for it, it was a shameful exhibition of cowardice. It ought to be distinctly understood that the recent cholera "scare" was a national discredit — discreditable if it was needless, more discreditable if it was necessary.

If we seek to discover how the bowel discharges of A may reach the mouth of B, we shall not have far to look. It is well worth our while to do this, for scientific prevention of disease must be based upon and guided by detailed knowledge of the ways in which disease is disseminated.

If by food we understand, as we may, both solids and liquids, such as bread and water, we may safely affirm that the commonest carriers of diarrhœal diseases, such as typhoid fever and Asiatic cholera, are undoubtedly *food and fingers*. Food may become contaminated by fingers, and fingers are only too easily contaminated by bowel discharges. I do not need to specify all the details, but every one knows how that most convenient organ, the human hand — the hand of the patient, the hand of the doctor, the hand of the nurse, the hand of the servant — may only too easily become fouled and contaminated with particles of excreta. Once this has happened it requires only a little carelessness, a little neglect, with no washing or with only imperfect or hasty washing, to leave the deadly particles upon the fingers, from which they may be rubbed off upon a loaf of bread, or fall into a pail of milk or rest upon a peach. From my experience with typhoid fever I am persuaded that in dealing with filth we are too apt to think of the more conspicuous public filth, and to forget the private and often invisible personal filth. It is of small avail to clean the streets of a city if typhoid or cholera patients continue to act as servants, cutting bread or handling fruit for others to eat, with fingers imperfectly cleaned.

I have perhaps said enough of solid food and fingers as agents or carriers of infection from diarrhœal diseases; but to show how disas-

trous such contamination may be when it extends to liquid foods like milk and water I may give an illustration by describing briefly one or two epidemics of typhoid fever due to infected milk, and a cholera epidemic due to an infected public water supply. There recently appeared among some of the best families in one of the best wards of a city of Massachusetts an epidemic of typhoid fever. The district was on high land, admirably drained and perfectly sewered. The public water supply was above suspicion, and was shared by all of the uninfected parts of the city in common with this district. Various vague theories were proposed to account for the disease. Some looked for the cause in a cemetery hard by; some pointed with positiveness to the well waters used by some of the families; some spoke of pestilence in the air, but failed to explain why the air in this region alone was pestilential. The State Board of Health, coöperating with the local board, made a careful inquiry, and found that the milk supply of this particular part of the city was infected; and the probable manner of infection was this:

In the family of the farmer who supplied the milk there had been typhoid fever. The excreta of the patients had been thrown into the privy. The contents of the privy had been spread upon a tobacco field. The cans of milk, after being nearly but not entirely filled, were submerged in a well in order to keep the milk cool and thus avoid the cost of ice. They were stoppered with wooden plugs, which were driven in hard. Nevertheless there was every reason to believe that the germs of typhoid fever had traveled from the tobacco field into the milk cans, and in the following manner: There was positive proof that in spite of the plugs the cans leaked around and through them; for on drawing up the cans and inverting them nearly one half of them were found to be leaking so much that drops of milk, and in some cases a stream of milk, ran out. But if milk could run out of cans thus stoppered, water could run in; and there was abundant evidence that it had so run in. It only remained to show how the well might have got infected. This well, however, was the only convenient source of cold water on the farm. It was a large well, covered with an old platform made of worn planks between which were wide cracks. There was one hole, two or three inches in diameter, in the platform. Pieces of manure were observed on the planks about to fall into the well. The curb was on one side, and the overflow water mostly fell upon the platform and ran back into the well, carrying with

it whatever lay on the planks near it. To this well, from the tobacco field, in rain or shine, with dry boots or wet, the men came for water or to attend to the milk. It was not proved that they had ever brought typhoid fæces from the field upon their boots, but there was reason to believe that they had done so, and had thus contaminated the well, while from the well the germs had leaked with the water into the milk.

In this way the bowel discharges probably passed from the patients on the lonely farm to the tables of the victims in the city. If the disease had been cholera instead of typhoid fever there would have been more deaths, but most of the phenomena would have been the same. If it had been cholera the whole country would have been in an uproar. But as it was "only" typhoid fever the dead were buried, the wounded have recovered and nothing more is heard of the "accident." In all twenty-five were killed and one hundred and thirty-five were wounded.

In another case, in a suburb of Boston, typhoid fever broke out among some of the best families. Four persons were killed and thirty were wounded. This "attack," also, was traced to infected milk, one of the milkmen himself having had the disease, and while suffering from diarrhœa having in all probability contaminated the milk at his central establishment.

I have said enough to show how milk, one of the most important and universally employed of food materials, may serve as a vehicle for diarrhœal diseases, such as Asiatic cholera and typhoid fever. A similar and equally instructive series of phenomena is true of drinking water. Here we have an excellent modern instance in the experience of the city of Genoa, Italy, in 1884. Cholera had appeared in Spezia, some fifty miles away; but after cholera had been raging in Southern Europe nearly two months Consul Fletcher of Genoa wrote to the authorities in Washington: "I do not believe the officials in any city in Europe could be more watchful or take more extreme precautions to ward off the epidemic than those in authority in Genoa. The result is that Genoa is in as healthy a state to-day as it is possible for human agency to make it, considering its peculiar construction and its proximity to the sea." The cholera was then but fifty miles distant; but for a month later the city retained its healthfulness, when suddenly the cholera appeared, and in one week one hundred and sixty-nine people died with this disease, nine tenths of whom were supplied by one of the three public water supplies of the city.

Upon investigation it was found that upon the stream which was the source of this water supply, thirteen miles from the city, between one thousand five hundred and two thousand laborers were at work upon a railroad, and in the previous week cholera had broken out among them. They washed their clothes in this stream, and some of their bowel discharges probably also found their way into it. On the tenth day after the cholera appeared in Genoa this source of water supply was cut off, and the portion of the city which it served was supplied from one of the other sources. The daily deaths from cholera immediately decreased, and in six days the number had fallen from thirty-eight to ten. The whole number of cases in the sections of the city supplied by the polluted water was four hundred and forty-four, and in the two sections supplied by public water supplies of unpolluted water the whole number was fifty-five.

In the autumn of 1890 the cities of Lowell and Lawrence suffered severely from an extensive epidemic of typhoid fever. The record of this unfortunate affair was not far from one hundred killed and one thousand wounded. The source of the disease was found to be discharges from the bowels of typhoid patients on a feeder of the water supply only a little more than two miles above the in-take of the Lowell waterworks. The journey of the germs from the bowels to the mouth was here perfectly evident.

I might extend almost indefinitely illustrations of this kind, showing how intricate, and yet how clear, the passage of germs from the intestine of A to the mouth of B often is, when food and fingers combine, or when food or drink become directly contaminated by sewage. The one indispensable requisite obviously is to *avoid the contamination of food and fingers.* It is probably safe to say that if the bowel discharges of the patients and the fingers of those who have to deal with cholera patients are properly disinfected all danger can be avoided and the disease confined strictly to its original appearance. The recent experience of New York City should teach us how foolish and unnecessary is any feeling of general alarm for those who have good water, good milk, clean servants and the other attributes of good living. It may be difficult to secure these; it certainly often is difficult, and for many it is at present impossible. But allowing that we should be most solicitous over these things let us not forget that the disease can be held in check by isolation and disinfection. If boards of health and other public functionaries do their duty; if every case that

appears is promptly and effectively isolated; if disinfection of the bowel discharges is thorough — in short, if the subject is treated in a scrupulously scientific manner the danger for well regulated communities can easily be reduced to a very small quantity.[1]

The citizens of Philadelphia, Chicago, Lowell, Lawrence and Albany on the other hand may reasonably feel much concerned about cholera, for their very large annual death rates from typhoid fever testify to the presence of bowel discharges in their supplies of drinking water. It is not generally known that the death rates from typhoid fever in Philadelphia and Chicago are and have long been more than twice as great as is that of Hamburg; so that there would be just cause for alarm in these cities if cholera should appear in America and should approach their water supplies.

All that I have said bears upon the question of quarantine. Isolation of patients, with disinfection of excreta, etc., we obviously must have. But quarantine such as we have recently seen in New York is in my opinion unnecessary and dangerous. In spite of it cholera got into New York, and while very likely more of it would have got in without the quarantine it is certain to my mind that it could and would have been as effectually suppressed as the smaller quantity was, and with less general suffering and alarm than the harbor quarantine as conducted produced. The community has it in its power at any time to suppress diarrhœal diseases if it will only isolate and disinfect, and it makes but little difference whether the disease is typhoid fever or Asiatic cholera.

For cities having polluted food and drink, or filthy ways of living, prolonged public or general quarantine is a desperate attempt at self-protection. For communities in good sanitary condition it is antiquated and clumsy.

Pettenkofer has given the key to the whole situation by saying that filth is like gunpowder, for which cholera is a spark. A community had better remove the gunpowder than try to beat off the spark; for in spite of their efforts, however frantic, this may at any time reach the powder and if it does is sure to blow them to pieces.

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[1] It is worth remembering that although about 18,000 persons in Hamburg had cholera in the recent epidemic about 604,500 of the inhabitants escaped. In the Lowell epidemic of typhoid fever 77,000 persons out of 78,000 remained unaffected. In other words only $\frac{18}{622}$ of the people in Hamburg had cholera, while $\frac{604}{622}$ escaped. In Lowell $\frac{1}{78}$ had typhoid fever and $\frac{77}{78}$ escaped.

ON THE LEAST NUMBER OF VIBRATIONS NECESSARY TO DETERMINE PITCH.[1]

By CHARLES R. CROSS and MARGARET E. MALTBY.

Received October 20, 1892.

The present paper is an extension of one by the same authors read at the meeting of the American Academy of Arts and Sciences held June 10, 1891, but not hitherto published. In the investigation described in it a method was employed which was originally proposed by one of the writers a number of years since in a paper read at the Philadelphia meeting of the American Association for the Advancement of Science, and published in abstract in the *Proceedings* for 1884, 33, 114.

In that paper attention was called to a defect in the method employed by Savart and Kohlrausch, from which they concluded that at least two complete vibrations are necessary to characterize pitch. This defect is a consequence of the character of the vibrations impressed upon the air by the blows produced by the teeth of the wheel of Savart or the comb of Kohlrausch.

The statement of this difficulty in the paper referred to is as follows:

"A cardinal defect in these methods of investigating the point in question is that the sound produced by a toothed wheel is very impure; first, because the noise of each separate impulse is mingled with the note produced by the coalescence of the separate impulses; and second, because the proper note, determined by the number of impulses, is itself far from simple in its character. To obtain results that are fully satisfactory the vibrations utilized should be pendular (sinusoidal) in their character. I see no reason in the nature of things why, if a single such simple vibration could be caused to fall upon the ear, it should not produce the sensation of a definite pitch. Rather, under the known action of the separate vibrating parts of the ear, we might

[1] Read at a meeting of the American Academy of Arts and Sciences, May 24, 1892.

suppose that even a small portion of a simple sound-wave, if it produced any recognizable sensation, would give a sound of a pitch corresponding to the length of the complete sound-wave, of which a part had impressed the ear."

The method devised in 1884 to remedy this defect consisted of a telephonic circuit containing two magneto-telephones, connected in the usual manner for the transmission of sound, and a circuit-making wheel. The latter was a wheel of vulcanite, furnished with a single narrow conducting strip of brass extending over a small arc of its circumference. Against the edge of the disk pressed a spring whose end was faced with platinum. If such a wheel is revolved at a uniform speed it is clear that once in each revolution the circuit between the two telephones is closed for a brief period, whose duration can readily be determined when the speed of the wheel and the angular extent of the conducting sector are known. If now the sound of a tuning fork giving simple harmonic vibrations actuates the transmitting telephone, the electrical undulation produced will be a sinusoidal one, and the air-waves produced at the receiver will be substantially similar in character, and continuous, provided the contact-making spring rests upon the metal sector of the wheel so as to complete the circuit. But if the wheel is revolved the electrical current is broken, except during a brief interval of time when the spring is in contact with the metallic sector; at which period, however, a current of brief duration is sent through the line and a correspondingly brief sound is produced at the receiver. Except in so far as self-induction and like phenomena act to prevent this, these brief electrical currents are still sinusoidal in character; and hence should give rise at the receiving telephone to waves or portions of a wave of sound which are sinusoidal in form, and should be perceived by the ear as a simple tone. In so far as the motion of the diaphragm of the receiver deviates from a simple harmonic one, either from disturbing electrical effects due to self-induction, or from the acoustic effect of the sudden pull and relaxation which occur when the circuit is made and broken by the contact-making spring, disturbing sounds will of course be added to the simple tone, which would otherwise be the only one produced. But unless the disturbing causes are excessive the pitch of the simple tone corresponding to that given by the tuning fork actuating the transmitter will predominate. And as a matter of fact these disturbances are slight, as is shown by the fact that there is no difficulty in transmitting speech through a circuit with a magneto-

transmitter at one end and a magneto-receiver at the other, when a rapidly revolving circuit-breaking wheel is interposed to interrupt the current as many as thirty times per second and upwards. At high rates of circuit-breaking the high-pitched note, due to the interruptions, is simply added to the vocal sounds transmitted.

In practice, however, a slight modification of the apparatus described was found to be necessary. As had been surmised when the apparatus was originally devised, clicks and scratches were heard in the receiving telephone when the current was rapidly made and broken by the revolving wheel. These often masked the proper sound of the fork. They seemed to be due to microphonic action at the contact of the spring and brass segment; and although we found that this could often be avoided by careful smoothing of the contact surfaces and proper adjustment of pressure, yet it was difficult to keep the apparatus in good adjustment. For this reason we substituted for the vulcanite wheel a brass wheel, having a single insulating segment of vulcanite. The receiving telephone, whose resistance was 115 ohms, was placed in derivation with the brass wheel, so that during the greater part of the time the receiving telephone was short-circuited and no sensible current passed through it, as the wheel had very slight resistance. But whenever the spring rested on the vulcanite segment the circuit through the wheel was broken; and hence a current passed through the receiving telephone and produced a sound. This arrangement obviated the difficulty just referred to, although care had to be taken to keep the contacts in good condition, as otherwise more or less of a scratching sound was introduced. In order to reduce the resistance of the short circuit through the wheel to a minimum, the current was caused to enter the wheel in the following manner: A disk made of sheet copper was attached to the axle carrying the brass wheel. The edge of the copper disk was amalgamated and its lower portion dipped into a trough of mercury, thus always securing a good contact. When the current was made to enter the wheel in other ways that were tried, as, for example, through a second spring, it was sometimes found that the resistance of the wheel circuit was not sufficiently low entirely to prevent the production of a sound by the derived current that under these circumstances constantly entered the receiving telephone. With the arrangement described the apparatus worked very satisfactorily, no trace of sound being audible in the receiving telephone except when the spring rested on the vulcanite strip, in which case the pitch

of the fork was perfectly clear. A key was inserted so that the sound produced at one or more successive contacts could be caused to enter the ear at will.

In our first experiments we employed as a source of sound a large tuning fork vibrating in front of a resonator and sounded by a hammer. But we found that this gave too feeble a sound for our purpose. We therefore made use of a small fork, which was set into vibration by a blow, and whose stem was lightly pressed against the disk of the transmitting telephone. A bit of wax placed at the end of the stem was found to be sufficient to prevent any chattering.

The particular form of magneto-telephone which was found most satisfactory as a transmitter, on account of its power, was a bipolar instrument having a large diaphragm, a special form made by the American Bell Telephone Co. For a receiver we found a very sensitive form of magneto-telephone made in Sweden best suited for our purpose.

The tuning forks employed were of small size, belonging to a Valentine and Carr tonometer. In all of the experiments cited the transmitter was placed in a distant room, so that no sound could possibly reach the ear through the air.

The speed of revolution of the brass wheel was read by a speed-counter, and from this it was easy to determine the period of time during which the contact-spring rested on the vulcanite sector.

The mode of observation adopted consisted in driving the wheel at a rate such that the difference in pitch between the brief sounds heard in the receiving telephone seemed to be distinct. One of the experimenters then sounded the forks successively, presenting them one after another to the telephone, and noting the order in which they were sounded. The listener at the receiver at the other end of the line noted the pitch heard, and these estimations were afterwards compared with the order as noted by the person at the transmitter. The speed was then altered, generally being increased, and a new set of experiments undertaken. In some cases, owing to bad transmission or for other causes, the listener felt uncertain whether the decision noted was correct, and in such cases the result was marked as doubtful.

In the first series of experiments only two notes were compared, which were an octave apart; one C_3 of 256 vibrations, the other C_4 of 512 vibrations.

The following figures will illustrate the general character of the results. The line marked "T" indicates the order in which the forks were actually sounded. The line "R" indicates the sound as noted by the listener at the receiving telephone. The number of complete vibrations of the lower note transmitted was 0.88; of the higher, 1.76. The doubtful estimates are followed by an interrogation mark.

TABLE I.

T.	C_3	C_3	C_4	C_3	C_3	C_3	C_4	C_3	C_3	C_4	C_4	C_4
R.	C_3	C_4	C_4	C_3	C_3	C_4	C_4	C_3	C_3	C_4	C_4	C_3

TABLE II.

T.	C_3	C_4	C_3	C_3	C_3	C_4	C_4	C_4	C_3	C_4	C_4	C_4
R.	C_3	C_4	C_4?	C_3	C_3	C_4	C_4	C_3?	C_3	C_4	C_4	C_3

In the first of the above series it will be seen that three estimations were wrong, nine right, none doubtful; that is, 25 per cent. were wrong, 75 per cent. right. In the second series one estimate was wrong, two doubtful and nine correct. That is, 8.3 per cent. were wrong, 16.7 per cent. doubtful and 75 per cent. correct.

The following tables give a synopsis of results reached with the C_3 and C_4 forks. Except in a very few cases twelve successive sounds were transmitted. The calculated duration of the sound is in all cases given in terms of the vibration period of both notes. The percentages are given to the nearest whole number. The first column contains the serial number of the experiment; the second (I), the percentage of incorrect estimates; the third (C), the percentage of correct estimates; the fourth (D), the percentage of doubtful estimates; the fifth (L) and sixth (H), the duration of the sound in terms of the period of the lower and higher notes respectively. The rates are always expressed in complete vibrations.

TABLE III.

Forks C_3 (256*v.*) C_4 (512*v.*)

No.	I.	C.	D.	L.	H.
1	33	50	17	0.88	1.76
2	25	75	0	"	"
3	8	92	0	"	"
4	8	75	17	"	"
5	0	100	0	"	"
6	0	100	0	"	"
7	0	100	0	0.80	1.60
8	8	92	0	"	"
9	0	100	0	0.73	1.46
10	0	100	0	"	"
11	17	75	8	0.52	1.04
12	42	58	0	"	"
13	27	64	9	0.42	0.84
14	50	42	8	"	"
15	33	67	0	"	"

The following results were obtained with intervals other than the octave:

TABLE IV.

Forks C_3 (256*v*). G_3 (384*v.*)

No.	I.	C.	D.	L.	H.
16	25	75	0	0.73	1.10
17	17	83	0	"	"
18	9	83	8	"	"
19	0	100	0	"	"
20	17	75	8	0.41	0.62
21	25	67	8	"	"

TABLE V.

Forks C_3 (256*v.*) E_3 (320*v.*)

No.	I.	C.	D.	L.	H.
22	33	50	17	0.40	0.50
23	42	58	0	"	"

In the sets of experiments Nos. 22, 23, the contact-spring failed to give even pressure; and it was noticed that the sound was very badly transmitted at certain times, which corresponded with those at which some of the erroneous estimates were made.

TABLE VI.

Forks C_3 (256*v*). C_3 (260*v.*)

No.	I.	C.	D.	L.	H.
24	58	25	17	0.40	0.41
25	25	75	0	"	"

It appears from the results given in the preceding tables that even with as small a fraction as $\frac{42}{100}$ of a vibration of the lower fork and $\frac{84}{100}$ of a vibration of the higher fork, when these had an interval of an octave, it was possible to distinguish one note from the other; and when the duration of the tone was greater, but still much less than two complete vibrations of the higher note, the distinction became easy. Nor was the distinction possible merely because of the possession of a clearly defined pitch by the higher note alone, whose period was shorter, so that a greater number of vibrations entered the ear in the time during which the circuit was completed through the telephone; which, it might be surmised, would enable one to distinguish it from the lower note even in the absence of any clearly defined pitch for this latter. On the contrary, the ear always recognized the existence of a distinct pitch with each note, even with the shortest duration of tone which was employed in the experiments; but, as would be expected, the ability to determine with certainty which of the two notes heard successively was the higher in pitch diminished as the duration of the sound diminished, and also as the interval between the two notes

became less. The results given in No. 24, Table VI, are curious; and at the time they were reached seemed to be due to a misjudgment as to which of the tones was the higher at the beginning of the experiment. Series No. 25 was made immediately afterwards; but the notes of the forks were carefully listened to separately, in order, if possible, to fix the pitch of each in the mind. As a result three fourths of the estimates were correct.

In another series of observations several forks were struck irregularly, and an attempt was made to state which fork of the set used was heard. The listener knew what were the forks composing the set.

With forks C_3, E_3, G_3, C_4, the results given in Table VII were reached. The separate series of observations are separated by horizontal lines. The table gives the name and rate of the forks used in the first two columns. In Column V is given the number of complete vibrations of each fork in the time during which the sound lasted in the particular experiment referred to; in Column O, the number of times each fork was sounded; and in Column P, the percentage of correct estimations.

TABLE VII.

Note.	Rate.	V.	O.	P.
C_3	256	2.4	8	100
E_3	320	3.0	13	85
G_3	384	3.6	11	73
C_4	512	4.8	6	83
C_3	256	1.3	9	77
E_3	320	1.6	14	64
G_3	384	2.0	16	69
C_4	512	2.6	7	71
C_3	256	1.2	10	80
E_3	320	1.5	16	62
G_3	384	1.8	15	80
C_4	512	2.4	9	77
C_3	256	1.0	22	64
E_3	320	1.3	21	57
G_3	384	1.5	22	84
C_4	512	2.0	11	82
C_3	256	1.0	14	62
E_3	320	1.3	37	57
G_3	384	1.5	18	88
C_4	512	2.0	8	87
C_3	256	0.9	28	68
E_3	320	1.1	36	64
G_3	384	1.4	27	63
C_4	512	1.8	9	100
C_3	256	0.9	25	72
E_3	320	1.1	34	65
G_3	384	1.4	31	71
C_4	512	1.8	14	85

Table VIII contains the results of similar observations made upon various series of notes.

TABLE VIII.

Note.	Rate.	V.	O.	P.
C_3	256	1.8	21	62
D_3	288	2.0	14	57
E_3	320	2.3	22	73
F_3—	340	2.4	24	71
G_3	384	2.7	22	64
A_3—	424	3.0	20	80
C_3	256	0.4	30	73
D_3	288	0.5	28	78
E_3	320	0.5	18	83
G_3	384	0.6	21	76
C_3	256	0.4	24	54
D_3	288	0.5	18	66
E_3	320	0.5	23	52
C_3	256	0.4	27	81
D_3	288	0.5	31	68
C_3	256	0.3	48	85
D_3	288	0.3	58	72
C_3	256	0.25	42	81
D_3	288	0.28	40	72

With the exception of the last three comparisons (which are inserted in this place because they were made in immediate connection with the preceding ones in the same table) the experiments whose results are given in Tables VII and VIII involve a more serious difficulty in estimation than do those previously considered; since, instead

of having to decide merely which of two notes is the higher, it is necessary to determine the musical relation of the various notes in the set sounded. Nevertheless, even with the shortest observed duration of the sound, the uniform preponderance of correct over incorrect or doubtful judgments is altogether too great to be the result of chance.

Moreover, even when the ability to determine which was the higher of two notes was lost, with briefer durations of the sound than those recorded in the tables, there was always a clear difference in the pitch of the two notes; that is, they did not seem like the same sound.

It will, of course, be observed that with the method used by us the element of memory enters, since the pitch of each sound must be carried in the mind. With a direct comparison of the sound heard through the telephone line, with that given by a tuning fork sounded by the observer at the receiving end, it is probable that a still larger percentage of results would be correct. This remains, however, to be actually proved by experiment.

It will also be noticed from the tables that in some cases it seems easier to judge correctly when the two tones make a small interval with each other than when they make a greater one. For example see the comparisons of C_3 and D_3 in Table VIII. That this is true when the greater interval is an octave might well be expected from the well-known liability to confuse notes of this interval; but it is true of notes of other intervals, which are not likely to be mistaken for each other.

It is our intention to extend these observations to briefer and longer durations of the sound, and to ascertain what number of vibrations is necessary to recognize the different musical intervals when these are entirely unknown beforehand. To obtain louder sounds it may be possible to use a microphone, although we have avoided this because of the great liability to disturbance of such a transmitter when strong vibrations actuate it. It has also occurred to us that we may use a sinusoidal electrical wave, generated by a peculiar form of alternating dynamo-machine, especially devised for the production of such a current by Mr. F. A. Laws, of the Rogers Laboratory. This will give a very loud and at the same time very pure tone. We also intend to study the comparative accuracy of estimation when each sound is heard only once, as in the observations detailed in this paper, with that which is obtained when the sound is repeated two or more times.

The question may be raised, regarding the method employed by us, whether the sound given out by the diaphragm was really of as brief duration as we have assumed it to be. It might be questioned whether the vibration of the diaphragm was not in fact prolonged; so that while the duration of the electric current was, for example, considerably less than the time occupied by two complete vibrations, yet more than two complete sound-waves were actually produced.

Of course, the instant the diaphragm ceases to be actuated by the electro-magnet of the receiving telephone, it will begin to assume its natural rate of free vibration; but how minute a fraction of a second will elapse before it ceases to move at substantially the rate impressed upon it might seem doubtful. But under the circumstances of many of our experiments, in which the duration of the current was from less than one half of a vibration up to one vibration, it certainly is not possible that the vibration should continue without entire change of rate and form for from four to eight times the duration of the forced vibration, which would have to be the case in order to have two complete sound-waves produced.

Furthermore, the damping effect of the magnet upon a telephone diaphragm is such as to prevent more than a very slight amount of persistence of vibration of any kind whatever. This is shown by the facts that the natural note of the diaphragm of a magneto-receiver is never perceived in the ordinary use of the instrument, and that the quality of the sound heard at the receiver is substantially the same, although wide variations may be made in the dimensions of its diaphragm. And if the after-tone of the diaphragm persisted after each pause sufficiently long to produce a recognizable sound of definite pitch, say for the period of two complete vibrations, this would necessarily be noticeable in the actual operation of the instrument — an effect which is not observed in practice.

Still further, in none of our experiments was there any note observed which corresponded in pitch to the natural note of the diaphragm — a note whose pitch was so high that it could not have failed to impress itself upon our attention had it been present to any material extent.

These observations seem to show that with ordinary telephone currents there is no material vibration of the diaphragm of the receiver after the forced vibration has ceased. With a stronger cur-

rent we might expect a certain continuance of the free vibration. What undoubtedly occurs is, that as soon as the electrical undulations cease to act on the receiver, its diaphragm begins to move at a different rate, passing to its free rate of vibration with great rapidity, and assuming this rate unless sooner brought to rest by acoustic and magnetic damping. This action would probably produce a noise of constant pitch, and distinct from the sound of the transmitting tuning fork.

But although for these reasons it appears extremely unlikely that the diaphragm could continue to vibrate with its rate substantially unchanged for more than a very minute fraction of a vibration, it nevertheless seemed desirable to ascertain by actual and direct experiment whether this is the case.

For this purpose the following apparatus was devised. A Lissajous' Comparator, with its vibration maintained electrically, was employed in the usual manner to study the motion of a very minute glass bead mounted upon the diaphragm of a magneto-receiver, through the coils of which was carried an alternating current. The glass bead was illuminated by sunlight or the electric arc. The rate of the fork was 128 complete vibrations per second. The alternating current could be caused to flow through the telephone coils for a definite time by the same circuit-making wheel which was used in the experiments already described. As we desired to use a far stronger current than that produced by the transmitting telephone, we made use of the alternating current from a transformer excited by a Thomson-Houston alternating current machine, making 128 complete alternations per second. The strength of the current through the coils was varied by a set of resistances made of incandescent lamps from a maximum of about 100 milliampères to a minimum of about 50 milliampères, an amount vastly in excess of any telephone current. When the circuit-making wheel was at rest and in the proper position the curves of Lissajous were clearly seen. The motion of the bead was very nearly a simple harmonic one, so that the curves observed were ellipses, passing into the limiting oblique, straight lines. Difficulty was met with from variations in the rate of alternation of the current, arising from very slight variations in the speed of the dynamo-machine, so that it was necessary to utilize such moments as were found to be available when the stead-

iness was sufficient. If the circuit-making wheel is in motion, it is clear that the appearance of the curve seen will depend upon the duration of the make, or rather upon the duration of the motion of the telephone diaphragm. If the harmonic motion of the diaphragm persists for a period equal to one complete vibration of the fork of the comparator (or, more strictly, for a period somewhat longer than this and depending upon the relative phase of the two vibrations when the current begins to flow through the telephone), it is evident that the curve should be a complete ellipse; but if the harmonic motion of the diaphragm lasts for a less time, then only a portion of an ellipse can be seen; and this will necessarily be deformed, as the curve must begin and end on the same vertical line; which is, of course, the vertical line given by the illuminated bead, as seen through the microscope of the comparator when no current is passing through the telephone.

The method has not yet been sufficiently perfected to give accurate, quantitative results; but with the rate of alternation as stated, viz., 128 per second, and with the strong current used, in some cases over 500 times as great as the ordinary current furnished by a magneto-transmitter, with a calculated duration of current of from $\frac{1}{512}$ of a second to $\frac{1}{256}$ of a second, in no case was a complete ellipse observed. In many cases the duration as determined from the curve appeared to be substantially the same as the calculated amount; and when there was a prolongation of the motion it was evident from the character of the curve that this was due to free vibration of the diaphragm.

From these results it would appear that the method used by us is not open to the objection referred to, and that there is really no material prolongation of the harmonic motion of the diaphragm of the telephone after the electric undulations have ceased to actuate it. Moreover, with the very weak current used in the transmission of the sounds of the tuning forks in our experiments, any such prolongation, if it existed, would of course be far less than with the strong current used in testing the point immediately under consideration. The effect of an increased rate of alternation ought to be studied, but the damping effect of the magnet would be greater at higher rates.

Some other experiments were made, however, with a current having 256 complete alternations per second, and of a rigorously simple harmonic form; but much difficulty was experienced in securing suffi-

ciently equable driving with such means as were available at the time of the experiment, so that further work in this direction had to be postponed until better conditions for steadiness could be secured.

The improbability of any prolongation of the sound-vibrations within the ear itself has been shown by Herroun and Yeo.[1]

A few experiments were made by us with a view of gaining some information regarding the free vibration of the telephone diaphragm after it was pulled to one side and suddenly released. For this purpose a direct current was substituted for the alternating current used in the experiments already described. The strength of the current was substantially the same as before. When the circuit-making wheel completed the circuit the diaphragm was suddenly drawn toward the magnet and released quickly when the circuit was broken. With a duration of current of about $\frac{1}{512}$ of a second the free vibration seemed to last for about the same period. With a duration of current of $\frac{1}{256}$ of a second the free vibration lasted from $\frac{1}{512}$ of a second to $\frac{1}{256}$ of a second. With a duration of current of $\frac{1}{128}$ of a second the free vibration often lasted as long as $\frac{1}{128}$ of a second. Under these circumstances the curve frequently showed clearly the character and rate of the free vibration of the diaphragm.

It is uncertain whether it will be possible to study the duration of the motion of the diaphragm by employing such currents as are ordinarily used in telephony, as the effect of these is so slight. In such a research it may prove advantageous to employ two telephones in circuit, each furnished with a mirror, and giving a fragment of one of Lissajous' curves when a beam of light is successively reflected from the mirrors, the momentary current being sent through the telephones by the circuit-making wheel; or a telephone with an objective carried by its diaphragm might be substituted for the fork of the comparator.

Two other methods have occurred to us of investigating the subject under consideration, which we hope to be able to make use of. The first of these is to employ the wave siren of Koenig, using a disk possessing only a single undulation of the sinusoid or a fraction of an undulation; or, for repeated sounds, an ordinary disk with only a definite fractional part of the undulations exposed to the action of the jet of air. Or perhaps the same result may be obtained by the use of

[1] Proceedings of the Royal Society, 50, 318.

a narrower slit than that ordinarily employed, so that the air jet shall cover only a portion of the sinusoid.

The second method is to employ a phonograph, on the wax cylinder of which are impressed sinusoidal undulations produced by the sound of a tuning fork. If only a single such undulation or portion of an undulation is allowed to remain, after removing the others by paring away the wax, the diaphragm will necessarily execute only a single harmonic vibration, or fraction of a vibration, when the cylinder is rotated in the ordinary manner.

ROGERS LABORATORY OF PHYSICS,
May, 1892.

REFORMS IN THE NOMENCLATURE OF ORGANIC CHEMISTRY.

By P. S. BURNS, Ph.D.

Read October 27, 1892.

One of the results of the intelligent development of organic chemistry has been to introduce into that science annually many thousands of new compounds. With no established rules as a foundation, the work of naming this vast body of compounds has necessarily introduced into the chemical nomenclature great confusion. As a matter of fact, for the future development of the carbon compounds, a reform in their nomenclature was almost imperative. This reform must not only take place in simplifying and reducing the number of names in present use, but must also provide names for those as yet unknown bodies whose existence we can so easily predict.

The International Congress of Chemists which met at Paris in 1889 first took up the question of a reform in the chemical nomenclature. After a limited discussion, realizing the magnitude of the subject, the Congress referred it to a committee[1] composed of chemists from Europe and America. The work of this committee, extending over a period of two years, was classified and arranged by a subcommittee — of which Friedel, of France, was president — and was presented by them to the new Congress which met at Geneva on the 19th of April, 1892, for approbation. The Congress consisted of the original committee, to which were added a number of other chem-

[1] The committee consisted of Béhal, Berthelot, Bouveault, Combes, Fanconnier, Friedel, Gautier, Grimaux, Jungfleish, Schutzenberger, from France; Graebe, Switzerland; Alexejeff and Beilstein, Russia; von Baeyer and Nölting, Germany; Lieben, Austria; Paterno, Italy; Franchimont, Holland; Armstrong, England; Istrati, Roumania; Calderon, Spain; Cleve, Sweden; Bonkowski-Bey, Turkey; Ira Remsen, United States; Mourgues, Chili.

ists, and the editors of the principal chemical journals. The following were present and took part in the discussions:

ENGLAND. — H. E. Armstrong, J. H. Gladstone, W. Ramsay.
FRANCE. — A. Arnaud, P. Barbier, A. Béhal, L. Bouveault, P. Cazeneuve, A. Combes, C. Friedel, A. Haller, M. Hanriot, A. Le Bel, L. Maquenne.
GERMANY. — A. von Baeyer, E. Fischer, E. von Meyer, E. Nölting, F. Tiemann.
AUSTRIA. — A. Lieben, Z. Skraup.
BELGIUM. — M. de Lacre.
HOLLAND. — A. P. N. Franchimont.
ITALY. — S. Canizzaro, A. Cosso, M. Fileti, E. Paterno.
ROUMANIA. — C. Istrati.
SWITZERLAND. — C. Graebe, P. A. Guye, A. Hantsch, D. Monnier, R. Nietzki, A. Pictet.

Other members of the commission who were not present at the Congress, but excused themselves by letters, were:

ENGLAND. — Crookes, Dewar, Frankland, Roscoe, Thorpe.
AMERICA. — Remsen.
GERMANY. — A. W. von Hofmann, Ladenburg, Liebermann, Victor Meyer, Ostwald, Pinner, Volhard, Witt.
FRANCE. — Ciamician, Gautier, Raoult, Schutzenberger.
RUSSIA. — Beilstein, Menschutkin, Mendelejeff.
TURKEY. — Bonkowski-Bey.
ROUMANIA. — Athanasesco.
ITALY. — Balbiano.
AUSTRIA. — Bauer, Lippmann.
SPAIN. — Calderon.
SWITZERLAND. — Piccard.
SWEDEN. — Cleve.
HOLLAND. — van Dorp, Goldschmiedt, Hoogewerff, Louy de Bruyn.
BELGIUM. — Henry.
PORTUGAL. — F. de Silva, Lourenco.

The elected officers of the Congress were as follows:

President. — Friedel.
Vice-Presidents. — Baeyer, Canizzaro, Gladstone and Lieben.
Secretaries. — Bouveault, Claparède, Nölting and Pictet.

The Congress held seven meetings, but was not able to accomplish all the work prepared for it to do. The discussions, held in the French language, were often lengthy, and many points were strongly contested in a spirited manner. Many chapters of the original work prepared by the Parisian committee were postponed for want of time. The topics acted upon were, however, carefully and thoroughly discussed, and resulted in the adoption of sixty-two resolutions. Many points, it will be seen, remain untouched by the Congress; nor is it to be

expected that the adopted resolutions will be found perfect. Many difficulties in their application will probably arise; these will have to be modified by the next Congress. But the underlying principles of the new terminology are certainly very exact, and of a nature to introduce into the nomenclature the unity and regularity which it has always lacked. The resolutions as adopted by the Congress are printed in *italics*, in order to separate them completely from the accompanying remarks.

The nature of the intended reform was first discussed. Bodies having similar constitutions have as a rule analogous functions, and it would therefore seem that they should all have a characteristic class name. It is often of great advantage, especially in instruction, to examine bodies from different standpoints, and to employ for this purpose as many different names. If, for example, we take prussic acid, and, looking at the analogy to hydrochloric acid which it possesses, consider it a product of the combination of H and the radical Cn, it would be given the name hydrocyanic acid. If, on the contrary, one views its relations to formic acid, it must be called formonitrile. These two names represent two different functions of prussic acid. In the same way we designate aniline with the two names phenylamine or amido-benzene, according as we wish more especially to regard it as an ammonia compound, or as a substituted benzene.

Emphasizing by many such examples the advantages arising from being able to express the different properties of the same body by various names, the Parisian commission asked that the reform in the nomenclature take place, not in giving to a body a single, unchangeable name, but in allowing the use of different names for the same body, according to the construction put upon it — the manner of giving these names to be regulated by fixed rules.

The German chemists, on the contrary, and especially Professor Baeyer, pointed out very strongly the great inconveniences which would arise, and which exist at present in the research libraries, from having so many synonyms. They asked that each body have a single, official name which shall appear in the indexes, tables and dictionaries, entire liberty being allowed authors to employ in their text such names as they choose, thus introducing the needed reform and retaining the terms familiarized by usage. After rather a long discussion this first resolution was adopted by the Congress:

1. *In addition to the customary name there will be established for*

each organic compound an official name. The official name is to be used in the indexes of books, journals, etc. The Congress expresses the wish that authors will give in their works — in parentheses — the official name at the side of the name which they have chosen.

In this resolution the Congress has not considered the task of reforming the chemical nomenclature, either written or spoken, but has desired to create a nomenclature that would serve solely as an index. In such a selection the questions of brevity and euphony are necessarily of secondary importance, the important point being that the official name shall picture to the mind the constitution of the body or the manner in which it could be synthesized. The principles which have governed the Congress largely in their work are as follows:

I. To found, where possible, the laws of the reformed nomenclature on the general principle — *substitution.*

II. To introduce into the names of all the derivatives of the same family a common *radical* which will indicate their derivation and origin.

III. By means of *prefixes* and *suffixes* attached to this radical to indicate the propensities of the groups which characterize the molecule.

IV. To construct the names from the chemical formula, separating if necessary each radical, and indicating its position according to a definite and constant order.

In order to name a compound according to the above principles we see that the *constitution* of the body must be well known. The question arose, what rules are to be followed in naming compounds whose constitution is either unknown or only incompletely determined? After a short discussion the Congress adopted the following proposition of C. Graebe on this point:

2. *To take up at present the naming of the compounds of known constitution, and to postpone until later those whose constitution is not well determined.*

The sphere of action of the Congress being thus exactly defined, the assembly took up the question of naming the hydrocarbons. The *official* nomenclature is to be based, as previously stated, on that of the substitutions. Each body is supposed to be derived from the fundamental hydrocarbon, by replacing atoms of hydrogen by radicals, or the hydrocarbon can be derived by replacing the radicals by atoms

of hydrogen. The first subject was, therefore, to name the hydrocarbons. In reference to them the Congress adopted the following resolutions :

3. *The termination* -ane *is adopted for all the saturated hydrocarbons.*

4. *The present names of the first four normal saturated hydrocarbons* (methane, ethane, propane, butane) *are retained. The names of the corresponding Greek numbers will be employed for those above four atoms of carbon.*

Here we note an important change. The above resolutions refer to the normal hydrocarbons. There will be in the future but one butane, one pentane and one hexane. Up to the present time these names applied indifferently to all the saturated hydrocarbons of 4, 5, 6, etc., atoms of carbon. These simple names are used where the carbon chain is straight. When the chain is branched the naming is regulated by the following resolution :

5. *The hydrocarbons with branched chains are regarded as derivatives of the normal hydrocarbons. They take the name of the hydrocarbon corresponding to the longest normal chain, and to this is added the name of the side chain.*

Thus the body

$$\begin{array}{c} CH_3 - CH - CH_3 \\ \quad\quad | \\ \quad\quad CH_3 \end{array}$$

is no longer a butane, but *methylpropane.* And so

$$\begin{array}{c} CH_3 - CH_2 - CH - CH_2 - CH_3 \\ \quad\quad\quad\quad | \\ \quad\quad\quad\quad CH_2 - CH_3 \end{array}$$

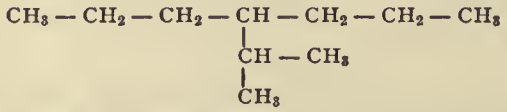
is not a heptane nor triethylmethane but *ethylpentane.*

We thus have to consider in the fat series a nucleus and a side chain ; the nucleus which plays the same rôle as C_6 in the aromatic compounds consists, as we have said, of the longest chain which it is possible to establish in the molecule. The residues which are attached to this normal chain are considered as substituting groups. Thus we speak of *ethylpropane* in the same way in which we have always spoken of ethylbenzene. When the side chain is itself branched, as occurs in the complicated hydrocarbons, how will the bodies be named, and how distinguish in the name the radicals which are attached to the side

chain from those joined to the principal chain? The difficulty has been covered by the proposition of A. Lieben, which was adopted:

6. *When a hydrocarbon radical is introduced into a side chain the terms metho, etho, etc., will be used instead of methyl, ethyl, etc., the latter prefixes being used when the substitution takes place in the principal chain.*

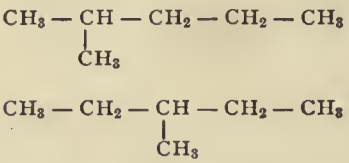
Thus



would have the name *metho-ethyl-heptane*, and not methyl-ethyl-heptane or isopropylheptane.

The naming of the hydrocarbons with branching chains being thus established on two very simple principles it remains to establish the manner of indicating the position of the side chains with respect to the normal chain. L. Bouveault proposed to indicate each atom of carbon in the principal chain by a letter of the Greek alphabet, and the side chain by the letter corresponding with the carbon atom where the substitution takes place.

Thus the two isomeric hydrocarbons

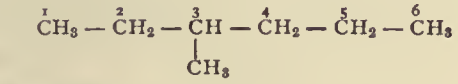


and

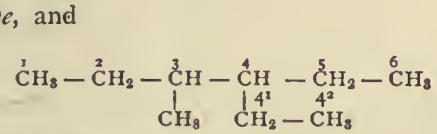
would be β methylpentane and γ methylpentane. This proposition found small favor with the Congress, which decided that the position of the substituting radical in the fat series, as well as in the benzene and naphthalene derivatives, shall be indicated not by letters as prefixes, but by numbers. The two hydrocarbons above would hence be named *methylpentane 2* and *methylpentane 3*. In order that the atoms of carbon in the side chain shall not be confounded with those of the normal chain the following resolution was adopted:

7. *The position of the side chains will be designated by numbers indicating to which atom of carbon in the normal chain they are attached. These numbers will commence at that end of the normal chain nearer the point of substitution. In case there are two side chains symmetrical to the principal chain the numbering will commence at the end which has the simpler side chain.*

Thus



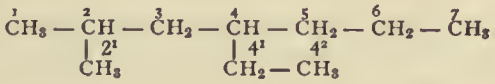
is *methyl-3-hexane,* and



is *methyl-3-ethyl-4-hexane.*

8. *The atoms of carbon of the side chain are designated by the same numbers as the atoms of carbon to which they are joined. Their position in the side chain is fixed by an indice number commencing with the point of attachment.*

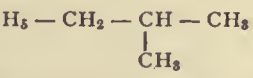
Thus:



9. *When two side chains are joined to the same carbon atom they are to be expressed according to their complexity — the simpler first.*

10. *The same manner of numbering will be adopted for the side chains joined to closed chains.*

For example



is 1^2 — *methopropylbenzene.*

Non-saturated hydrocarbons. The Parisian committee proposed to name the non-saturated hydrocarbons according to their valence; that is, by the number of atoms of chlorine or bromine which they are capable of absorbing. The saturated hydrocarbons have the termination -ane, and so A. W. Hofmann proposed, in an analogous manner, to designate the non-saturated hydrocarbons as follows:

Bivalents by the termination -ene; C_4H_8 being butene.
Tetravalents by the termination -ine; C_4H_6 being butine.
Hexavalents by the termination -one; C_4H_4 being butone.
Octavalents by the termination -une; C_4H_2 being butune.

The Congress, however, was in favor of the proposition of A. von

Baeyer, that is, to name them according to the number of double or triple bonds which they contain, and adopted his proposition as follows:

11. *In the non-saturated hydrocarbons with open chains having a single double bond, the termination* -ene *is to be used corresponding with* -ane *in the saturated hydrocarbons; when two double bonds occur the termination* -diene, *and when three the termination* -triene, etc.

We will thus speak of *ethene* for ethylene; *propene* for propylene; *hexadiene* for diallyl.

12. *The names of the hydrocarbons with triple bonds shall end correspondingly in* -ine, -diine, -triine. Acetylene is thus *ethine*, and dipropargyle becomes *hexadiine.*

13. *If there are simultaneously double and triple bonds the terminations* -enine, dienine, etc., *will be used.*

14. *The carbon atoms in the non-saturated series are to be numbered in a manner corresponding with the saturated. In a case of ambiguity, or in absence of side chains, the end carbon atom nearer the highest bond will be numbered 1.*

Thus:

$$\overset{1}{C}H_3 - \overset{2}{C}H(\overset{}{C}H_3) - \overset{3}{C}H_2 - \overset{4}{C}H = \overset{5}{C}H_2$$

$$\overset{5}{C}H_3 - \overset{4}{C}H_2 - \overset{3}{C}H_2 - \overset{2}{C}H = \overset{1}{C}H_2$$

$$\overset{1}{C}H \equiv \overset{2}{C} - \overset{3}{C}H_2 - \overset{4}{C}H = \overset{5}{C}H_2$$

15. *When necessary the position of the double or triple bond will be indicated by giving the number of the first atom of carbon to which it is joined.*

For example:

$CH_2 = CH - CH_2 - CH^3$ butene 1.
$CH_3 - CH = CH - CH_3$ butene 2.

In addition to the saturated hydrocarbons there exists a number of compounds in which the molecule is formed by the union of 3, 4, 5 or 6 CH_2 groups united into a closed chain. These have the names trimethylene, tetramethylene, pentamethylene and hexamethylene. These names, which resemble those of the non-saturated hydrocarbons, were not approved by the Congress, which adopted the proposition of H. E. Armstrong in regard to them.

16. *The saturated hydrocarbons with a closed chain will take the names of the corresponding saturated hydrocarbons of the fatty series and the prefix* -cyclo.

Thus:

cyclopropane.

Before leaving the hydrocarbons the Congress adopted the following proposition in regard to their derivatives:

17. *The same manner of numbering the hydrocarbons is to be followed in all their products of substitution.*

It is understood that in all cases where the numbering is not fixed by the position of the side chain it is to commence at the extremity of the principal chain nearer the point of substitution.

For example:

$CH_2Cl - CH_2Cl$ 1.2. dichlorethane.

$CH_3 - CHCl_2$ 1. dichlorethane.

$CH_3 - CH_2 - CH_2 - CH_2 - CH_2Br$ 1. bromopentane.

$CH_3 - CH(CH_3) - CH_2 - CH_2 - CH_2Br$ 2. methyl-5-bromopentane.

$CH_3 - CH_2 - CBr(CH_3) - CH_2 - CH_3$ 3. methyl-3-bromopentane.

$CH_3 - CH_2 - CH(CH_2Br) - CH_2 - CH_3$ 3. methyl-3[1]-bromopentane.

The Congress next took up the naming of various simple functions.

18. *The alcohols and phenols are to take the names of the hydrocarbons from which they are derived, followed by the suffix* -ol.

19. *In the case of the polyatomic alcohols and phenols, there will be inserted between the name of the fundamental hydrocarbon and the suffix the words di, tri, tetr, etc., to indicate the atomicity.*

20. *The name mercaptan is to be abolished, and this function expressed by the suffix* -thiol.

According to these rules methylalcohol would have the name *methanol;* ethylalcohol, *ethanol;* allylalcohol, *propenol;* glycerine, *propanetriol;* mannite, *hexanehexol;* and the common mercaptan would be *ethane-thiol*, etc.

For naming the simple ethers as well as the sulphur ethers the Congress adopted provisionally the proposition of F. Beilstein:

21. *The oxyethers take the names of the hydrocarbons of which they are composed, separated by the word* -oxy.

Ordinary ether will have the name *ethane-oxy-ethane;* amylether, that of *pentane-oxy-ethane;* anisol, *benzene-oxy-methane.* For the sake of convenience the more complicated hydrocarbon is named first.

22. *The sulphur ethers are named in the same manner as the oxy-ethers, using the syllable* -thio; *the disulphur ethers by* -dithio *and the sulfones by* -sulfone.

For example:

$C_6H_5 - S - C_2H_5$ benzene-thio-ethane.
$C_6H_5 - S - S - C_6H_5$ benzene-dithio-benzene.
$C_6H_5 - SO_2 - C_6H_5$ benzene sulfone-benzene.

The method of naming the aldehydes, ketones and quinones is regulated by the following resolutions:

23. *The aldehydes are characterized by the suffix* -al *being added to the name of the hydrocarbon from which they are derived. The sulphur aldehydes by the suffix* -thial.

For example:

CH_3CHO *ethanal.*
CH_3CSH *ethane-thial.*

24. *The ketones are characterized by the suffix* -one. *The diketones, triketones, thioketones are designated by the suffixes* -dione, -trione, -thione.

Thus:

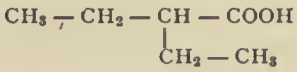
CH_3COCH_3 propanone.
$CH_3COCH_2CH_2CH_3$ pentanone .2.
$CH_3CH_2COCH_2CH_3$ pentanone .3.
$CH_3COCH_2COCH_3$ pentandione .2. 4.

25. *The present nomenclature for the quinones remains unchanged.*

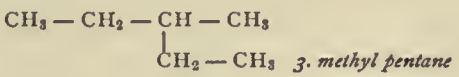
For the naming of the organic acids two propositions were introduced for the consideration of the Congress. According to one of these — the report of Parisian commission — the acids would take the name of the hydrocarbon containing the same number of carbon atoms as the acid, followed by the suffixes -oic, -dioic, -tetraoic, etc., according

to the number of carboxylic groups. L. Bouveault proposed, on the contrary, to consider the carboxyl as a substituting group, and to regard the acids as derivatives of hydrocarbons having a smaller number of carbon atoms. The name of the acid would thus be taken from that of the inferior hydrocarbon by adding the suffixes -carbonic, -dicarbonic, -tricarbonic, etc., this latter method being in practical agreement with the present method for the aromatic and pyridene acids.

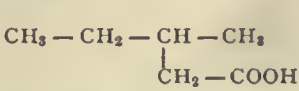
Succinic acid would thus be called by the first method, *butane-dioic acid;* and by the second, ethane dicarbonic acid 1, 2. These two methods would both come under the fundamental principles admitted by the Congress, and both present advantages and disadvantages. In the first we can see an advantage in preserving intact in the name the original skeleton of the hydrocarbon, and thus not separating the acids from the alcohols from which they are derived by simple oxidation. This method could be applied admirably to the naming of the normal acids of the fat series, but leads necessarily to ambiguity in the acids from hydrocarbons with branched chains. Thus, for example, the acid



the fundamental hydrocarbon of which is



would be named by the first method *3-methyl-pentanoic-acid.* This name should evidently be reserved for the acid derived from the normal acid by substituting a methane group in place of an atom of H on carbon number 3, viz.:



The name *pentane-3-carbonic-acid,* which would be given to it by L. Bouveault, avoids this confusion.

By the second method we would regard acetic acid as a derivative of methane, and call it *methane carbonic acid.* This name sounds to us a little strange; it is clear, on the contrary, that the method is better adapted for naming the acids in which the carboxyl is in the side chain.

The great advantage, however, is seen in the uniformity which could thus be adopted in all parts of organic chemistry as well for the aromatic as for the fat series. After a long discussion of the two propositions that of the Parisian committee was adopted by a vote of twenty-four for and ten against, with the restriction that it should be applied only to the fat series. The second principle is to be applied to the aromatic acids, so that we shall have the two methods of naming acids. The adopted resolutions in regard to the acids are as follows:

26. *The name of the monobasic acids of the fatty series will be that of the corresponding hydrocarbon with the suffix* -oic. *The polybasic acids will be designated by the terminations* -dioic, -trioic, etc.

27. *In the acids of the fatty series the carboxyl group will be considered an integral part of the carbon skeleton.*

Thus formic acid is methanoic acid.
acetic acid is ethanoic acid.
oxalic acid is ethanedioic acid.
succinic acid is butanedioic acid.

he proposition of C. Graebe, asking to preserve the present names of the first four monobasic acids, was lost.

28. *The acids in which* one *or* two *atoms of sulphur have replaced the corresponding atoms of oxygen in the carboxyl group are named as follows: if the sulphur is singly bound to the carbon atom it will be designated by* -thiol; *if the bond is a double one, by* -thione.

Thus:

CH_3COSH ethanethiolic acid.
CH_3CSOH ethanethionic acid.
CH_3CSSH ethane-thione-thiolic acid.

29. *In the monobasic acids with a normal chain the carbon atom in the carboxyl group is numbered* 1. *In all other cases the same order is to be followed as in the fundamental hydrocarbons.*

Thus:

$$\overset{4}{C}H_3-\overset{3}{C}H_2-\overset{2}{C}H_2-\overset{1}{C}OOH$$

$$\overset{1}{C}H_3-\overset{2}{C}H(\overset{}{C}H_3)-\overset{3}{C}H_2-\overset{4}{C}OOH \qquad \overset{1}{C}H_2=\overset{2}{C}H-\overset{3}{C}H_2-\overset{4}{C}OOH$$

30. *The present manner of naming the salts and compound ethers remains unchanged.* Thus: ethanoate of ethyl, methanoate of calcium, ethane dioate of potassium.

31. *The anhydrides of the acids will be named in the same manner as at present.* Thus: ethanoic anhydride.

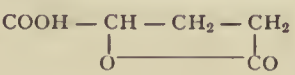
32. *The lactones will be designated by the suffix* -olide. *The position in the principal chain of the alcoholic oxygen with respect to the carbonyl group will be indicated by the Greek letters,* α, β, γ, δ, *beside the customary numbers.*

Thus:

pentanolide, 1. 4
or
γ-pentanolide 1. 4.

The acid lactones derived from the dibasic acids will be named as the lactones, and will take the suffix -oic.

Thus:



pentanolidoic acid.

With respect to the nitrogen derivatives the Congress adopted most of the propositions prepared by the Parisian commission. They are but slight modifications of the present nomenclature.

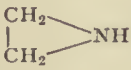
33. *Amines: no change in the ammonia compounds.* For example, ethylamine. *When the group* NH_2 *is considered as a substituting one, it will be expressed by the prefix* amino, *instead of* amido.

Example:

CH_2NH_2COOH amino-ethanoic for amido-acetic acid.

The bodies in which the bivalent group NH *closes a chain of positive radicals will be called* imines.

Thus:



etheneimine.

The committee propose to name the group NH_2 amigene, *and the group* NH imigene.

34. *The nomenclature now in use for the phosphines, arsines, stibines and sulphines will be retained.*

35. *The compounds derived from hydroxylamine by replacing the hydrogen of the hydroxyl group are to be designated by the suffix* -hydroxylamine.

Thus:

$C_2H_5 — O — NH_2$ is ethylhydroxylamine.

The oximes are to be named by adding the suffix -oxime *to the name of the corresponding hydrocarbon.*

$CH_3 — CH_2 — CH_2 — CH = NOH$ butanoxime 1.
$CH_3 — CH_2 — C = NOH — CH_3$ butanoxime 2.

36. *The names* amides, imides, amidoxims *are retained.* They are to be added only to the name of the hydrocarbon and no longer to that of the acid.

Thus:

CH_3CONH_2 is *ethanamide* instead of acetamide,

$\begin{matrix} CONH_2 \\ | \\ CONH_2 \end{matrix}$ is *ethane-diamide* instead of oxamide,

$\begin{matrix} CH_2CO \\ CH_2CO \end{matrix} > NH$ is *butanimide* instead of succinimide.

37. *The term* urea *is retained; it will be employed as a suffix for the alkylated urcas, while the derivatives by acid substitution will be the* ureides. For example, ethylurea, ethanoylureide. *The bodies derived from two molecules of urea are designated by the suffixes* -duirea-duireides. *The acid ureides take the name* ureic acids. *The names* uramic *and* uric *are not to be used.*

38. *Amidines: this suffix is retained.*

Example:

$CH_3 — C\begin{matrix} = NH \\ \diagdown NH_2 \end{matrix}$ ethanamidine.

39. *The customary term guanidine is retained, but different guanidines are named as substitutions of diamido-carbo-imidine.*

40. *Betaines take the suffix -taïne.*

$(CH_3)_3 \begin{matrix} N — CH_2 — CO \\ \diagdown\!\!\diagup O \diagdown\!\!\diagup \end{matrix}$ ethanoyltrimethyltaïne.

41. *Nitriles: for the derivatives of the fatty series where the group* Cn *is part of the principal chain, the name of the hydrocarbon is taken,*

followed by the suffix -nitrile. *When the* Cn *group is in a side chain, no rule has been established. In the aromatic series the prefix* -cyano *has been adopted.*

CH_3Cn ethane-nitrile.
C_6H_5Cn cyanobenzene.

We see here a complication resulting from the adoption of articles 26 and 27 relating to the nomenclature of the acids; here we have a double method of naming, according as the body belongs to the fat or aromatic series, and no rules for bodies having branching chains.

42. *Carbylamines: the present nomenclature is to be used.*

43. *Isocyanic ethers: suffix* -carbonimide.

$CO=N-C_2H_5$ ethyl-carbonimide.
$CS=N-C_2H_5$ ethyl-thione-carbonimide.

The prefix *-thione* is used here after article 28, since the sulphur is bound to the carbon by a double bond. The terms essence of mustard, mustard oil, etc., used as generic terms for designating bodies of the formula $CS=N-R$ will be dispensed with.

44. *Cyanates: this name is reserved for the true ethers, which by saponification yield cyanic acid or its hydration products. The name* sulphocyanate *is replaced by that of* thiocyanate.

45. *Nitro-derivatives: there will be no change from the present nomenclature.* We will still say nitro-ethane, nitro-benzene, etc.

46. *Azo compounds: the terminations* azo, diazo, hydrazo, azoxy *are unchanged.* The manner of using these terms, however, is somewhat different, the same principle being employed as in the nomenclature of the ethers.

Thus:

$C_6H_5-N_2-C_6H_5$ benzene-azo-benzene.
$C_6H_5-N_2-C_6H_5-N_2-C_6H_5$ benzene-azo-benzene-azo-benzene.

The same principle as for naming the ethers.

47. *The symmetric hydrazines are considered as hydrazoic derivatives, and named as such. Other hydrazines are designated by the names of the radicals of which they are composed, followed by the suffix* -hydrazine.

$C_6H_5-NH-NHCH_3$ benzene-hydrazo-methane.
$\begin{matrix} C_6H_5 \\ CH_3 \end{matrix}\!\!>\!N-NH_2$ phenylmethylhydrazine.
$CH_3-C_6H_4-NH-NH_2$ methophenylhydrazine.

48. *The name of the hydrazones is formed by replacing the terminations* -al *or* -one *of the aldehydes and ketones by the suffix* -hydrazone. *The term* -ozazone *is replaced by that of* dihydrazone.

$CH_3 - CH_2 - CH = N - NH - C_6H_5$ propanhydrazone 1.

$CH_3 - C(CH_3) = N - NH - C_6H_5$ propanhydrazone 2.

The Congress now considered the more difficult problem of naming the bodies with complex functions. A. von Baeyer proposed to name these by the same rules as those adopted for the bodies with simple functions. This method, very logical in principle, unfortunately so clashes with our language as to be untenable. Thus the body

$$COH - CH_2 - CHOH - CO - COOH$$

which is at the same time aldehyde, alcohol, ketone and acid, would have the name *pentan-ol-al-on-oic acid.* The large number of suffixes which would thus be necessary in order to name many bodies soon becomes ludicrous, and leads to a complication difficult of pronunciation. Then again it is difficult to insert the numbers indicating the positions of the substituting groups. The Parisian commission sought to solve these difficulties by having what is considered the most important or characteristic function expressed by the termination, the other functions being designated by prefixes which would express the nature of each group. Thus the above body would be named by this system, 2 keto -3 oxy -5 aldehydo-pentanoic acid, or other endings for the prefixes as might be decided upon. This makes the names very long, but allows the insertion of the numbers, and so far as simply furnishing a name for an index answers very well. We can see, however, a great inconvenience which would arise from the introduction of two distinct modes of designation for each function, as would be the case in this system, depending on whether we consider any group an important or secondary function. And then, again, who is to decide which is the important group in any compound? After a long discussion of the subject the Congress, realizing that neither the Parisian commission nor they individually had sufficiently studied the subject, concluded to return the question for the examination of the Parisian commission, and to that effect passed the following resolution:

49. *Further discussion of the nomenclature of the compounds with complex functions is postponed; the question is referred to the Inter-*

national Committee for preparing a plan to be presented at the next Congress. The committee are charged to consider the exigencies of the spoken nomenclature, as well as a terminology applicable for an index.

Here we see that the Congress has desired to look after the spoken nomenclature, as well as one intended solely for an index.

RADICALS.

For naming the hydrocarbon radicals the Congress adopted the following resolutions:

50. *The names of the monovalent radicals derived from the hydrocarbons by the elimination of an atom of hydrogen have the termination* -yle. *This ending replaces the suffix* -ane *for the saturated hydrocarbon radicals; it is added to the full name of the unsaturated hydrocarbons.*

Thus:

$CH_3 — CH_2 —$ ethyle.
$CH_2 = CH —$ ethenyle.
$CH \equiv C —$ ethinyle.

51. *The alcoholic radicals, that is to say, those radicals derived from the alcohols by splitting off an atom of hydrogen directly bound to carbon, are named by adding* -ol *to the radical of the corresponding hydrocarbon.*

Thus:

$— CH_2 — CH_2OH$ ethylol.
$— CH = CHOH$ ethenylol.

52. *The aldehydic radicals are named in a corresponding manner to the alcohol radicals by using the termination* -al.

Thus:

$— CH_2 — CHO$ ethylal.

53. *The acid radicals which still possess an acid character, that is to say, the radicals resulting after eliminating an atom of hydrogen bound to carbon, are named in a manner similar to the alcohol and aldehyde radicals by adding to the corresponding hydrocarbon radical* -oic.

Thus:

$— CH_2 — COOH$ ethyloic.

The acid radicals, on the contrary, which are derived by eliminating the hydroxyl of the carboxylic group are named by giving them the termination -oyle *in place of* -oic *of the acid.*

Thus:

$CH_3 — CO —$ ethanoyle.

54. *Where two radicals are joined to the same carbon atom, the more complicated is given first.*

Example:

Pentylmethylamine, phenylethylhydrazine.

AROMATIC SERIES.

At the last meeting the Congress took up the nomenclature of the aromatic compounds. This subject, so vast and important, had consequently in many parts to be treated in a very superficial manner. The three principal ideas introduced for naming the aromatic hydrocarbons were:

I. The naming of the fundamental hydrocarbons.

II. The nomenclature of the side chains and the substituting groups.

III. The manner of indicating the positions of the side chains or substituted groups with respect to the nucleus.

No resolution was adopted on the first point. The former Congress of Paris, in 1889, had decided to replace the names of benzine and naphthaline, which names have the fault of being those which have been reserved for the organic bases, by those of *benzene* and *naphthalene*. The French chemists have in fact adopted these two names, but in Germany and in America the term benzol is still largely used. The term benzol should not be used as it is too near to the ending characteristic of the alcohols. M. Graebe has furnished an idea which it is to be regretted the Congress of Geneva did not adopt. He proposed to give to benzene and naphthalene the names *phene* and *naphtene*. These two names, beside their brevity, would allow of the application of the rules already adopted. *Phenyle* and *naphtyle* would be the corresponding radicals, and *phenol* and *naphtol* the hydroxyl compounds. It is to be hoped that the next Congress, which will have to fix names for the aromatic compounds, will consider this proposition. On the second point, concerning the nomenclature of the side chains and substituted groups, the following resolution was adopted:

55. *In the aromatic derivatives and in all the bodies with a closed chain the side chains will be considered as substituting groups.*

$C_6H_5 - CHO$ methylal-benzene.

$C_5H_3N(COOH)_2$ pyridine-dicarbonic acid.

The third point, the manner of indicating the position of the sub-

stituting groups, has been very finely set forth by A. Combes. An outline of his paper is as follows:

Originally Kékulé proposed to designate the summits of the hexagon which was to represent the benzene molecule by the numbers 1, 2, 3, 4, 5, 6, and also to indicate by symbolic numbers the relative positions of the substituting groups.

Koerner proposed for easily naming the bi-substitution bodies to designate the compounds of the 1, 2 series by the prefix *ortho;* those of the series 1, 3 by *meta;* and of 1, 4 by *para.* This mode of designation is to-day accepted universally, and permits of a clear manner of designating all the bi-substitution derivatives. When the number of substitutions is greater than two the problem of naming the divers isomeric compounds by the present existing method is infinitely more difficult, and especially is this true when the substituted radicals are different. Owing to the perfect symmetry of the benzene molecule it is impossible to select any one of the points of the hexagon and call it number 1 (as in the open chain the end carbon atom was number 1). As a result of this perfect symmetry compounds could be represented by many different symbols. For example oxytoluic acid could be represented by the six symbols

COOH (1) CH_3 (3) OH (4)	CH_3 (1) COOH (3) OH (6)
CH_3 (1) OH (2) COOH (5)	COOH (1) OH (4) CH_3 (5)
OH (1) CH_3 (2) COOH (4)	OH (1) COOH (4) CH_3 (6)

according as we assign number 1 to the group

COOH, CH_3 or OH

If now one of the summits of the hexagon could be singled out from the others, and that one called number 1, we would introduce a dissymmetry in the molecule that would enable us, after adopting a few simple rules, to construct without hesitation the formula for any polysubstituted derivative of benzene. With this object in view A. Combes proposed the following resolutions, which were adopted:

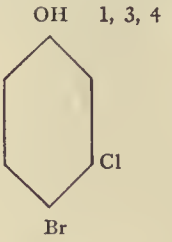
56. *The atoms of carbon of the benzene nucleus are numbered from 1 to 6.*

57. *In the polysubstitution derivatives of benzene we will indicate the substituting group in which the atom bound directly to carbon has the smallest atomic weight, as number 1.*

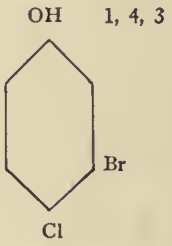
58. *The place 1 being thus fixed, the other groups will be named*

in the order of the increasing atomic weight of the atoms in those groups directly bound to carbon, those of the smaller atomic weights coming first.

Thus phenolchlorobromide would be written

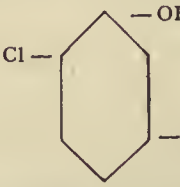


and its isomer



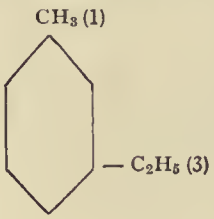
In case of identity of two atoms bound directly to the carbon skeleton we are to consider the other atoms in the group; the group having the smallest molecular weight will be given first.

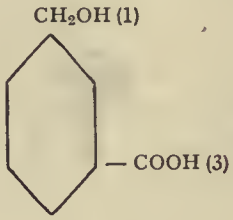
Thus:



will be 1, 3, 6.

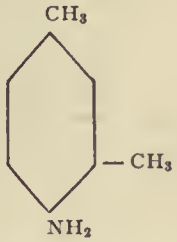
In the cases where there exist several side chains we give first places to those which contain only one atom of carbon. For classing the chains we consider if they are derivatives of the group CH_3, *by replacing 1, 2 or 3 atoms of hydrogen, and give number 1 to the group which causes the least increase of molecular weight.*





59. *When the same substituting group is repeated several times the index 1 will be given to the group, which will make the index of the second substituting group the smallest.*

Thus:



Aminodimethylbenzene
1, 3, 4;
and not as could be written,
1, 3, 6.

60. *When two benzene rings are joined directly or indirectly the numbers of the ring given last will be accented.*

Mr. Armstrong offered a general scheme for naming all the bodies referred to a non-saturated closed chain or ring, such as pyridine, pyrrol, furfurane, etc. Bouveault also offered a proposition on the same subject. For want of time to discuss the subject it was postponed until the next meeting, and there was adopted in reference to this point the following proposition:

61. *The discussion of the nomenclature of the bodies with closed non-saturated chains is postponed until the publication of the ideas of Armstrong will allow the commission to compare them with those of Bouveault.*

The Congress closed its discussions with the following proposition concerning their application:

62. *The Congress invites the editors of the chemical journals to apply the principles which have been adopted.*

The Congress in passing this last resolution did all that it could as a body to introduce into general use the principles it had adopted. When we consider that the men composing the Congress represented the highest talent in the chemical world it seems probable that we may look forward to a rapid introduction of the new nomenclature. This desirable result would, however, be much hastened if chemists in general would take hold of this subject.

It may be interesting to many to know that the next edition of Beilstein's *Organische Chemie* will be printed as far as possible in accordance with the new nomenclature.

UPON THE DIFFICULTY WITH WHICH WATER PARTS WITH ITS DISSOLVED OXYGEN.

By AUGUSTUS H. GILL, Ph.D.

Received November 30, 1892.

In the course of a series of determinations of dissolved oxygen for the Massachusetts State Board of Health, it was observed that a water almost completely aërated could be heated considerably above its saturation point without a corresponding loss in oxygen. As apparently the fact had been unnoticed, it was investigated, with the following results:

As is well known, with a saturated solution of a solid, a depression of the temperature causes its almost immediate precipitation, more especially if it be shaken, or particles of the same are in contact with it. With gases, the molecules of which are so free to move, one would suppose the same phenomena would occur with even greater ease and rapidity, as Ostwald says, "by rise in temperature, liquids are completely freed from dissolved gases." This, however, takes place very slowly, as is shown by the following experiments:

METHOD OF PROCEDURE.

The calibrated bottles holding about 250 cc. were filled from the laboratory tap by a rubber tube passing to the bottom of the bottle, the water being previously allowed to run for some time to insure a representative sample. In taking this sample a quantity of water was allowed to run sufficient to fill the bottle about sixteen times. The temperature was taken by placing a delicate thermometer in the bottle while it was being filled. The method followed for the determination of oxygen was that of L. W. Winkler;[1] after being filled, some of the bottles were left standing in the room, the temperature of which was 22° C., and others were placed in the water bath at 30° C.

[1] Berichte der deutschen chemischen Gesellschaft, 21, 2843.

SERIES I.

Time of heating.	Temperature.	cc. of oxygen.	Per cent. basis of 9°.	Per cent. basis of 22°.	Per cent. basis of 30°.
0	9°	6.760	83.87	....	....
		6.822	84.60	....	....
		6.832	84.73	....	....
7 hours.	22°	6.530	80.98	106.8	....
24 hours.	22°	6.249	77.50	102.2	....
4 hours.	30°	6.540	81.13	....	124.5

After being heated four hours at 30°, the water was still supersaturated, less than 4 per cent. of oxygen having been given off; this was even less than was given off on heating seven hours at 22°, it requiring twenty-four hours at this temperature to bring it down to the point of saturation.

In Series II the oxygen seemed to leave the water even more slowly, six and one half hours' exposure at 30° causing a loss of about 6 per cent., and forty-eight hours' exposure at 22° causing but 2–3 per cent. loss.

SERIES II.

Time of heating.	Temperature.	cc. of oxygen.	Per cent. basis of 9°.	Per cent. basis of 22°.	Per cent. basis of 30°.
0	9°	6.823	84.61	....	....
		7.016	87.02	....	....
		6.955	86.26	....	....
48 hours.	22°	6.639	82.33	108.6	....
		6.832	84.72	111.8	....
		6.528	80.76	....	124.2
6½ hours.	30°	6.416	79.58	....	122.1

The foregoing experiments were performed with 85 per cent. aërated water; 92 per cent. gives up the oxygen rather more slowly as might be thought, as the following series indicates.

SERIES III.

Time of heating.	Temperature.	cc. of oxygen.	Per cent. basis of 5.9°.	Per cent basis of 22°.
0	5.9°	8.031	92.33	....
		8.071	92.80	....
24 hours.	22°	7.772	89.32	127.2
		7.643	87.86	125.0
48 hours.	22°	7.088	81.47	116.0
		7.083	81.42	115.8
		7.451	85.65	....
7½ hours.	33°	7.405	85.22	....

The results of this experiment confirm the observations made in the winter, that the waters then received being cold and nearly saturated hold on to the oxygen very tenaciously.

To ascertain if the shape of the vessel exercised any influence upon the rapidity with which the oxygen was given off, experiments were made using bottles with necks one and one half inches in diameter instead of half an inch:

Water as drawn. Percentage of aëration.	Exposed 24 hours at 22° in bottles of	
	1½ inch neck.	½ inch neck.
89.39	83.04	88.79
89.99	83.85	89.00
92.33	79.37	87.86
92.80	....	....

These show, as was expected, that the greater the surface the more rapidly the oxygen leaves the liquid, making a difference of about 5 per cent.

The results given in these series do not indicate particularly closely agreeing determinations, by no means within the limit of accuracy of the method. This is due to the fact that it is well-nigh impossible,

even after the water has been allowed to run from ten to fifteen minutes, to draw consecutive samples of water that are of the same composition. Mr. F. S. Hollis, one of the water analysts of the city of Boston, has observed this same fact, and collects the samples by allowing the water to run through a T tube into each bottle. In the following series the bottles were all filled from the same large bottle of water, the contents of which had been thoroughly mixed.

Time of heatiog.	Temperature.	cc. of oxygen.	Per cent. basis of 11°.	Per cent. basis of 22°.	Per cent. basis of 30°.
0	11°	6.409	83.34	....	....
		6.396	83.16	....	....
		6.396	83.16	....	....
		6.376	82.89	....	....
24 hours.	22°	6.250	....	102.3	....
12 hours.	30°	5.255	....	....	100.2
		5.297	....	....	100.8

This shows a much better agreement of the results among themselves than those of Series I, II and III, and substantiates the conclusions there drawn.

That water does undergo changes in its oxygen contents in the pipes is well shown in the following series: during the night there had been very little drawn; it was allowed to run five minutes, and samples taken consecutively at five different times.

Time.	Temperature.	Per cent. aëration.
9.02 A.M.	27.5°	46.73
9.07 A.M.	7.5°	80.86
9.17 A.M.	6.°	80.30
9.37 A.M.	5.5°	82.31
10.07 A.M.	5.5°	83.80

Another experiment showed after the water had been running fifteen minutes, 89.4 per cent. aëration; after running thirty minutes, 92.5 per cent. This would indicate that the composition of a water

coming through any considerable distance of small pipe is not constant as regards oxygen contents until perhaps two hundred liters have been drawn, or it has been allowed to run from a half inch faucet for half an hour.

The results of the foregoing experiments would indicate:

1. The necessity of knowing the initial temperature of a water in order to report "percentage of saturation."

2. That a water can be heated ten degrees above its saturation point for twenty-four hours — in some cases forty-eight hours — without giving up entirely the excess of oxygen; this takes place with greater rapidity from a wide-mouthed vessel.

3. That it is impossible to draw two consecutive samples of water from the same faucet of exactly the same oxygen contents. This variation is undoubtedly due to changes of pressure in the pipe system. Possibly this statement may be true of other constituents.

Laboratory of Sanitary Chemistry and Gas Analysis,
November, 1892.

TESTS ON THE TRIPLE ENGINE AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

By C. H. PEABODY and E. F. MILLER.

Received December 8, 1892.

The tests described in this paper, and for which the data and results are given in the accompanying tables, were a part of the regular work in the laboratory of steam engineering at the Massachusetts Institute of Technology for the school year of 1891–92. They are reported here because they are believed to be of value to the engineering profession; and in order that they may receive the attention which we believe they deserve it is proper that we should state our reasons for our confidence in their accuracy and correctness, especially as student work is sometimes considered of little value. The observers, who were students of the third year class, and who had already received instruction from preliminary tests of like nature but which are not reported here, were in divisions of about twenty, each under the direction of the instructor in charge of the laboratory, aided by three competent assistants. All the instrumental work, such as weighing water, reading gauges and taking indicator diagrams, received close and unremittent attention from the instructors. The measurements of areas and pressures on the indicator diagrams were made independently by two observers, and checked. All the calculations for all the tests were carried through by the instructing force, and thoroughly checked. Finally, it is believed that an inspection of the results of the tests, as given in the table and the accompanying diagrams, will show that the variations of results of comparable tests are well within the limit of the errors of observation. Thus the tests No. 1 and No. 2 may be considered to be duplicates; their consumptions of heat are 233 and 237 B. T. U. per horse power per minute; the difference, 4 B. T. U., is less than two per cent. Other pairs, such as No. 3 and No. 4, No. 5 and No. 6, show a like satisfactory result, while none of the results reveal notable discrepancies.

The engine, which has already been presented to the attention of this society, has provision for jacketing the barrels and heads of the cylinders and the intermediate receivers. The tests given here are the beginning of a series which is intended to show what advantage, if any, may be obtained by jacketing the various organs of the engine with steam. While the engine is provided with the necessary piping, valves, etc., for running in various combinations, and while the cranks may be set at various angles, it was run in the usual way for a triple engine, with the steam passing in succession through the high-pressure cylinder, the first receiver, the intermediate cylinder, the second receiver and the low-pressure cylinder, and then to a surface condenser; and the cranks followed at 120° in the order of high, intermediate and low pressure. The priming in the steam near the throttle valve was 1.1 per cent. for all the tests. The steam in the jackets was at boiler pressure, and was assumed to have the same quality. The steam condensed in the condenser was collected and weighed in tanks; the drain from the several jackets was caught, and measured in graduated receivers. All the tests were one hour in duration.

Some of the principal dimensions of the engine are given in the following table:

HIGH.

Diameter	8.99 in.
Stroke	30.00 in.
Diameter piston rod	2.19 in.
Clearance in per cent. of piston displacement	C.E. 9.76 H.E. 8.83
Piston displacement	C.E. 1.037 cu. ft. H.E. 1.102 cu. ft.

INTERMEDIATE.

Diameter	16.01 in.
Diameter rod	2.19 in.
Stroke	30 in.
Clearance in per cent. of piston displacement	C.E. 10.9 H.E. 10.4
Piston displacement	C.E. 3.430 cu. ft. H.E. 3.495 cu. ft.

LOW.

Diameter	24.063 in.
Diameter rod	2.16 in.
Stroke	30 in.
Clearance in per cent. of piston displacement	C.E. 12.27 H.E. 12.18
Piston displacement	C.E. 7.831 cu. ft. H.E. 7.894 cu. ft.

The surface condenser into which the engine exhausts is used for a variety of laboratory work, and has various pipes entering it, each with its valve. The spindles of these several valves, though tight under steam pressure, could not be prevented from allowing air to leak into the condenser at varying rates. Thus the vacuum was never satisfactory and varied from day to day; it was, however, possible to maintain it steady during a single engine test. Since the performance of a multiple expansion engine depends largely on the vacuum, it has been considered advisable to reduce all the tests to the same vacuum. The table gives the steam per horse power per hour, and the B. T. U. per horse power per minute actually consumed. Also the B. T. U. per horse power per minute calculated on the following assumptions: (1) that the vacuum was always 26 inches of mercury, and (2) that the mean effective pressure for the low-pressure cylinder was increased (or diminished) by the difference between the actual and the assumed absolute pressure in the condenser.

The tests may be arranged in five groups, as follows:

1. The first nine tests recorded in the table were made with steam supplied to the jackets on the three cylinders of the engine, but not to the receiver jackets. This series is the most complete and gave the best economy. The results of the tests are plotted on Figure 1 with the per cent. of cut-off on the high-pressure cylinder for abscissas and with the B. T. U. per horse power per minute for ordinates, the latter beginning at 230 on the diagram in order that a large vertical scale may be used without undue size for the plate. The curve drawn to represent these tests shows that the best performance of the engine is obtained with the cut-off on the high-pressure cylinder at 30 per cent. of the stroke, and that longer cut-off and more power requires more heat per horse power. The cut-off on the intermediate and low-pressure cylinders for this series, and indeed for all the tests reported, was set to give an expansion in the high-pressure and intermediate cylinders down to or nearly to the back pressure lines for those cylinders, and thus to avoid loops or appreciable drop to the receiver. It will be noticed that the vertical displacement of the points representing tests No. 1 to No. 7 from the curve is not more than $2\frac{1}{2}$ thermal units; the displacement of the tests No. 8 and No. 9 is about 5 thermal units; but they, with No. 7, sufficiently determine the general direction of the curve, which has the well-known form of such curves, showing a gradual reduction of heat consumption as the

cut-off is shortened till a minimum is reached, and beyond that a large and rapid increase in the heat consumption.

The action of the steam jackets and the interchange of heat between the steam and the cylinder walls is roughly indicated by the per cent. of steam at cut-off and release. In the high-pressure cylinder we find the familiar initial condensation and reëvaporation, the action being more marked as the cut-off is shortened. The efficiency of the steam jacket is shown by the fact that the steam is fairly dry at release for all the tests, and with the cut-off at 35 per cent. the steam is

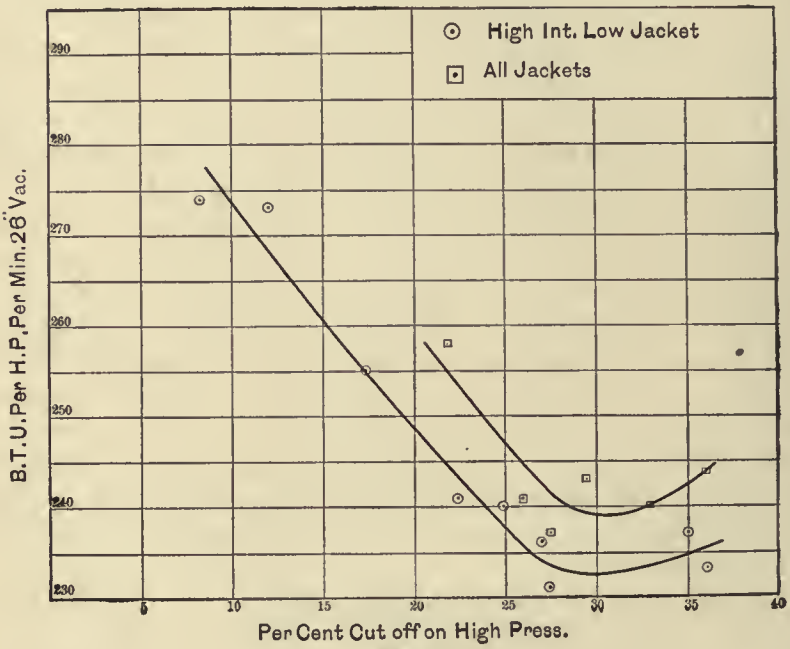


Fig. 1.

not very wet even at cut-off. The action in the intermediate cylinder, though less marked and less regular, is similar to that in the high-pressure cylinder. For all the tests, except No. 8 and No. 9, the steam is wetter at release than at cut-off in the low-pressure cylinder, showing a condensation during expansion — a most unexpected result, the more especially as that cylinder was thoroughly jacketed with boiler steam, and there can be no question that there was an energetic action of the jacket, as evinced by the large condensation in that jacket by column 10 of the table.

2. The tests Nos. 10 to 15 inclusive were made with steam in the jackets of all the cylinders and in the jackets of the intermediate receivers. The results are also plotted on Figure 1, and are represented by a curve, which shows definitely that the minimum consumption for that series is attained at about 32 per cent. cut-off on the high-pressure cylinder. The vertical displacement of none of the points representing these tests from the curve exceeds 4 thermal units. A comparison of the columns 20, 21, 30, 31, 40 and 41 of the table shows that with all the jackets supplied with steam, including those on the receivers, the steam passing through the cylinders becomes progressively dryer in its course through the engine, and is substantially dry and saturated at release from the low-pressure cylinder. It should be remarked that calculations of the quality of the steam in the low-pressure cylinder showed superheating, as indicated in the table, at cut-off for all of this group of tests, and at release for two of them. The superheating, however, was in no case large, and in most cases may be within the limits of error of the test. The same action will be observed for the low-pressure cylinder in these tests that was noticed in the first group, namely, that the steam is dryer at cut-off than at release.

The most important conclusion arrived at from a comparison of this group of tests with the first group is that the application of heat in the jackets was carried to an excess. The excess in the heat consumption at the minimum for this group of tests over that for the first group is only 6 thermal units, but the difference, though small, is distinct. Probably a reduction of pressure of the steam in the jackets of the second receiver and low-pressure cylinder will be found to give a favorable result.

3. The group of tests numbered from 16 to 24 in the table was intended to show the action in the individual jackets. A comparison of tests No. 1 and No. 16 shows that the heat consumption was in the proportion of

$$233 : 269 :: 1 : 1.11,$$

or that the saving from the use of steam jackets was about ten per cent., which is what was found to be the gain from jacketing both simple and compound engines in the well-known Revenue Steamer tests by the engineers of the United States Navy.

While the tests in the first and second groups show a progressive drying of the steam on its way through the engine, due to the action of the jackets, these tests show that the steam, which in the high-

pressure cylinder is nearly as dry as in the first groups, becomes progressively wetter — a fact that has been reported for triple expansion marine engines.

A comparison of tests No. 18, No. 19 and No. 20 with tests No. 21 and No. 22 shows that it does not make much difference in economy whether the steam jacket, if only one is used, be placed on the intermediate or the low-pressure cylinder. A comparison of all five tests mentioned with the tests No. 16 and No. 17 shows a distinct advantage from the use of a jacket on either of these cylinders. This

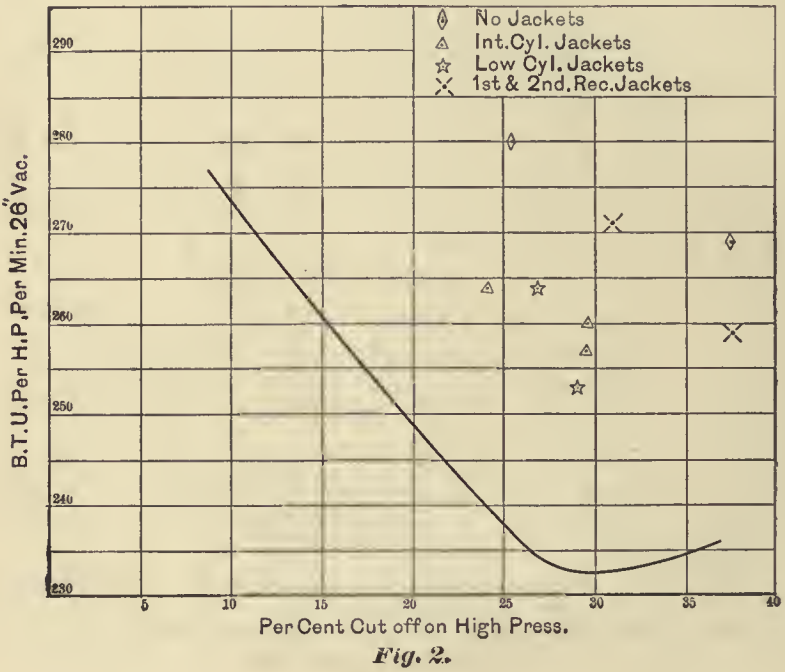


Fig. 2.

last is more apparent from Figure 2, on which are plotted the results of all the tests of this third group, together with the curve showing results of the first group of tests, which is given as a sort of standard of comparison.

It is clear from investigation of the table and from Figure 2 that the use of jackets on the intermediate receivers for tests No. 23 and No. 24 does not give much advantage, certainly not so much as the use of a jacket on either the intermediate or low-pressure cylinder. This conclusion, of course, holds only for this particular type of re-

heater, and on our engine; the advantage, if any, to be obtained from the use of other types of reheaters, such as coil or tubular reheaters, and on other types of engines, can be found only by proper tests on such engines.

4. The main interest of the fourth group of tests, which are plotted on Figure 3, together with the curve representing the first group, is to see which end of the organs of the engine should be supplied with steam jackets, *i.e.*, whether jacket should be applied to the cylinders and receiver when the steam contained is at high pressure, or to the cylinders and receiver when the steam is at low pressure.

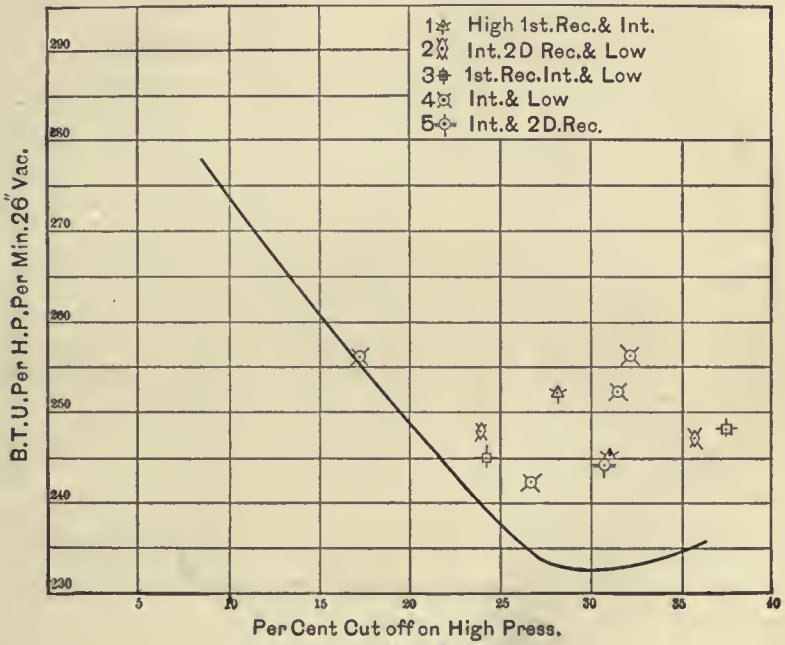


Fig. 3.

While the tests are neither sufficiently numerous, nor so related as to settle the question, they appear to indicate that it makes little difference which system is used. The comparison of tests No. 29 and No. 30 with No. 27 and No. 28 shows that it makes little difference, if any, which receiver is supplied with a jacket in combination with jackets on the intermediate and low-pressure cylinder. Again, the tests No. 31 to No. 34 with jackets on the low and intermediate cylinders show on the whole very similar results at 30 per cent. cut-off for the high-pressure cylinder.

5. The last group is made up of a miscellaneous lot of tests that were made somewhat at random to find the effect of various combinations in addition to those already discussed. They are plotted on Figure 4, together with the curve representing the first group of tests. Together with the tests in the fourth group they show that the jacketing of the three cyclinders of the engine (without jackets on the receivers) gives the best result, unless it may be at very early cut-off on the high-pressure cylinder, for which arrangement the economy of the engine is decidedly worse than for a cut-off at about one third stroke.

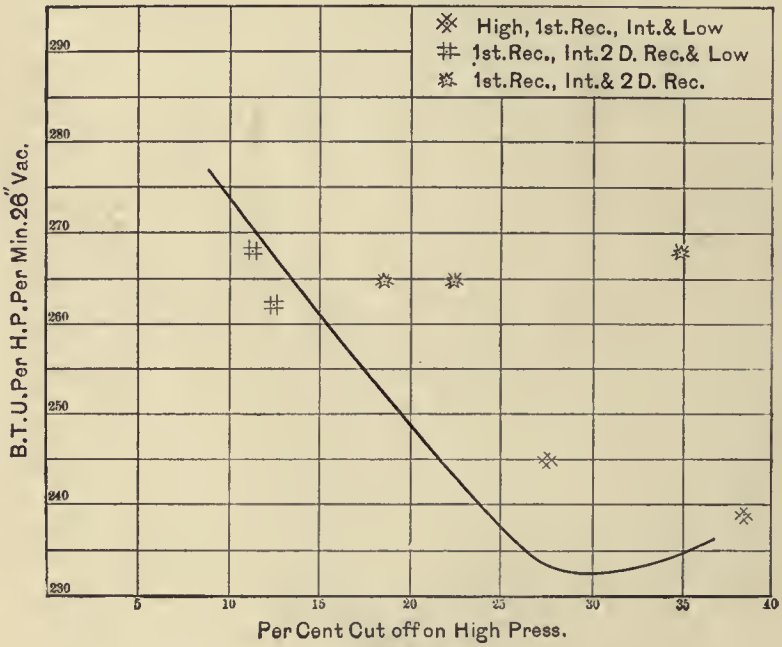


Fig. 4.

The tests No. 38 and No. 39 with jackets on the two receivers and the intermediate and low-pressure cylinders are the only ones that are represented on the diagram by points that are below the curve of the first group. The importance of tests at so early a cut-off on the high-pressure cylinder has already been shown to be small.

Though it is too early to announce any definite general conclusion from this series of tests we have gathered a general idea that it makes but little difference where steam jackets are used on an engine, provided the jacketing is carried far enough and not too far.

TESTS ON THE TRIPLE ENGINE AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

	Date. Month and Day. 1892.	Horse Power.	Steam per Horse Power per Hour.	Steam through Cylinders per hour.	Water by Jackets per Hour.					Revolutions per minute.	Boiler Pressure (Gauge).	Vacuum in Condenser (inches Mercury).	Barometer.
					High-Pressure Cylinder.	First Receiver.	Intermediate Pressure Cylinder.	Second Receiver.	Low-Pressure Cylinder.				
1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	3/4	140.79	13.82	1647.8	53.3	.....	140.9	.....	104.1	89.93	146.2	24.1	29.8
2	3/15	137.99	13.88	1640.5	40.9	.....	144.9	.....	88.8	90.60	147.0	24.7	30.3
3	3/7	125.37	13.74	1420.5	41.0	.....	138.5	.....	117.2	91.93	146.9	24.5	29.9
4	3/14	123.89	13.74	1405.5	44.7	.....	137.3	.....	114.9	91.55	146.7	25.4	30.1
5	3/22	114.67	14.36	1329.5	45.3	.....	138.3	.....	133.7	92.37	146.6	24.5	30.7
6	4/22	105.32	14.48	1251.0	40.4	.....	132.4	.....	101.0	84.87	145.2	24.3	30.1
7	4/5	103.50	14.71	1210.0	41.9	.....	130.2	.....	139.6	93.15	146.0	26.0	30.2
8	5/13	78.32	15.13	891.5	53.9	.....	132.0	.....	107.5	86.70	147.0	27.4	30.5
9	5/12	67.45	16 00	800.0	52.3	.....	122.4	.....	104.2	87.55	146.7	26.0	30.1
10	4/22	154.16	14.46	1782.5	47.6	84.5	113.5	96.0	105.1	91.20	143.7	24.7	30.2
11	4/22	145.15	14.15	1633.5	46.7	82.7	116.3	69.5	105.2	91.40	143.6	25.0	30.2
12	4/21	137.06	14.34	1545.0	46.4	86.6	118.0	74.9	94.4	91.82	143.2	25.2	30.5
13	3/29	128 82	14.13	1449.0	20.6	67.9	129.0	47.7	106.2	91.83	147.1	24.7	30.3
14	4/19	125.86	14.14	1409.5	45.2	62.6	115.1	66.9	80.9	92.17	145.5	25.5	30.4
15	4/21	120.25	14.58	1358.0	46.1	92.2	96.9	55.7	104.5	92.57	143.7	26.4	30.6
16	2/29	109.74	15.34	1683.0	....	.....	.....	.....	.....	87.77	145.1	24.8	30.4
17	2/26	89.27	15.81	1411.0	....	.....	.....	.....	.....	88.95	146.8	25.3	30.3
18	2/25	115.21	15.10	1601.5	....	.....	138.0	.....	.....	88.87	146.2	24.7	30.3
19	4/21	116.50	14.92	1576.0	....	.....	162.3	.....	.....	90.15	144.9	24.9	30.5
20	2/23	104.50	15.54	1105.0	....	.....	113.0	.....	.....	88.87	146.6	24.5	30.7
21	2/19	115.10	15.31	1500.6	....	.....	.....	.....	182.3	88.40	145.4	25.2	30.4
22	2/18	108.29	15.72	1510.5	....	.....	.....	.....	192.0	88 16	144.9	24.3	30.2
23	3/24	127.70	15.87	1801.0	....	81.7	.....	143 2	.....	90.88	145.3	22.6	30.3
24	4/11	119.93	16.08	1692.0	....	89.4	.....	147.6	.....	91.23	146.2	24.1	30.1
25	4/25	121.88	14.38	1508.0	44.1	86.7	113.5	.....	.....	83.52	145.4	24.7	30.6
26	4/26	113.08	14.36	1384.5	44.4	78.3	116.5	.....	.....	84.05	146.6	26.0	30.5
27	4/15	131.54	14.69	1656.0	....	.....	90.8	88.3	91.6	89.83	144.4	24.0	30.0
28	4/12	114.17	14.48	1337.5	....	.....	171.0	54.1	91.1	91.40	146.2	25.5	30.3
29	3/10	151.05	14.74	1851.0	....	87.9	142.7	.....	144.4	90.63	146.2	24.0	29.9
30	3/8	124.48	14.41	1456.5	....	99.0	118.8	.....	119.9	91.80	145.5	25.1	29.8
31	4/28	122.33	14.87	1510.0	....	.....	149.7	.....	159.1	83.32	145.6	25.4	30.3
32	3/3	127.97	14.58	1564.5	....	.....	168.0	.....	133.0	87.90	145.2	25.4	30.0
33	3/1	120.32	14.34	1451.0	....	.....	159.0	.....	115.0	88.68	145.2	24.6	30.3
34	4/29	99.23	14.78	1195.0	....	.....	147.4	.....	124.6	83.32	146.4	25.9	30.2
35	3/21	120.99	16.25	1438.3	....	.....	173.5	136.0	.....	88.88	145.2	23.1	30.5
36	3/11	151.10	14.37	1779.5	38.1	72 0	138.9	.....	142.6	90.48	145.8	23.5	29 6
37	3/17	130.30	14.29	1530.8	44.1	68.7	102 7	.....	115.7	91.42	146.6	25.0	30.4
38	5/10	88.57	15.15	1032.5	....	79.4	100.9	43.0	86.2	86.10	146.8	26.2	30.4
39	5/9	83.69	15.53	997.0	....	72.0	86.4	44.0	100.6	86.53	145.9	26.1	30.4
40	3/31	128.47	16.08	1734.0	....	113.9	142.0	75.8	.....	90 65	147.2	24.0	30.7
41	4/1	102.98	16.25	1358.8	....	97.3	162 2	55.0	.....	92.55	146.2	24.0	30.7
42	4/4	101.67	15.49	1316.0	....	80.7	128.6	50.0	.....	93.05	146.1	25.4	30.3

TESTS ON THE TRIPLE ENGINE AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.— *Continued.*

HIGH-PRESSURE CYLINDER.

	Initial Pressure.	Per Cent. of Cut-off.	Pressure at Cut-off.	Pressure at Release.	Pressure at Compressioo.	Per cent. of Steam in the Cylinder at Cut-off.	Per. cent. of Steam in the Cylinder at Release.	M. E. P. Crank.	M. E. P. Head.	Horse Power.
1	15	16	17	18	19	20	21	22	23	24
1	140.5	36.1	136.6	48.1	60.6	.906	.938	46.10	47.76	39.41
2	141.3	35.0	133.1	49.1	58.3	.874	.989	46.11	46.44	39.13
3	140.6	27.3	131.6	37.2	45.3	.841	.935	42.53	47.04	38.49
4	142.3	27.0	131.5	36.3	42.4	.839	.943	46 98	47.96	40.58
5	142.3	25.0	127.3	31.5	33.9	.833	.918	50.00	50.40	43.17
6	140.5	21.9	129.0	28.6	26.0	.759	.869	51.37	52.01	40.95
7	142.0	17.4	128.7	21.9	27.6	.728	.830	41.06	45.17	37.55
8	142.9	12.0	128.6	16.1	19.9	.720	.885	37.16	35.58	29.42
9	140.8	8.3	135 8	12.6	17.1	.697	.880	33.12	30.82	26.08
10	142.1	36.1	133.1	46.4	53.0	.834	.881	51.47	51 09	43.65
11	142.0	32.8	130.8	42.1	48.3	.832	.897	49.86	50.27	42.75
12	141.2	29.3	128.9	36 9	39.5	.809	.883	51.11	52.97	44.63
13	143.0	27.5	129.6	36.5	45.5	.816	.905	45.73	46.69	39.62
14	140.7	25.9	124.1	33.9	39.4	.778	.907	42 36	45.88	38.00
15	141.5	21.9	129.0	27.5	29.3	.757	.847	47.36	50.03	42.12
16	140.9	37.5	135 6	50.8	51.5	.905	.956	49.56	54.31	42.60
17	142.9	25.4	138.7	34.7	35.2	.808	.895	46.27	50.45	40.20
18	142.4	29.6	127.0	40.2	44.6	.717	.818	44.28	46.14	37.52
19	140.4	29.5	128.0	38.2	43.4	.767	.807	46.17	46.50	38.99
20	141.2	24.1	134 6	33 6	36.6	.687	.776	45.22	46.70	38.14
21	141.5	29.1	128.3	37.7	37.2	.802	.889	52.29	52.11	43.06
22	140.3	26.8	131 3	37.1	41.6	.742	.878	44.60	48.18	38.21
23	140.5	37.7	127.0	49.3	55.3	.825	.898	47.56	48.20	40.62
24	141.4	30.9	135.2	40.1	45.9	.802	.850	52.74	52.30	44.72
25	141.5	30.8	133.0	40.4	41.5	.811	.882	53.78	54 22	42.10
26	141.0	28.2	129.7	35.4	40.9	.802	.873	48.90	50.08	38.83
27	137.7	35.7	121.9	42.0	56.9	.810	.860	44.02	43.92	36 86
28	142.7	23.9	128.0	31.0	36.5	.793	.902	45.46	46.32	39.11
29	141.2	37.5	129.6	48.9	58.0	.813	.868	47.54	47.23	40.08
30	140.0	24.3	123.5	31.8	35.5	.731	.833	46.42	46.93	40.00
31	140.9	32.2	135.4	44.4	52.3	.943	.912	46.25	45.91	36.64
32	141.1	31.4	130.5	41.6	48.7	.816	.886	43.04	45.61	36.40
33	140.6	26.6	123.1	34 1	32.9	.781	.865	46.88	52.40	41 16
34	144.0	17.3	128.5	21.1	24.6	.677	.766	47.16	45 23	36.75
35	141.4	30.7	129.6	40.4	50.0	.776	.850	47.80	48.20	39.83
36	144.9	38.5	132.8	52.8	65.8	.858	.938	45.51	43.43	37.53
37	139 8	27.6	132.5	36 7	42.7	.793	.850	48.10	46.64	40.40
38	141.9	12.5	131 9	17.8	21.7	.656	.812	37.48	36.46	29.70
39	139.4	11,3	134.9	17.1	21.2	.653	.824	35.80	33.46	27.93
40	143.0	34.8	131.6	48.7	69.2	.802	.896	38.22	38.85	32 61
41	141.7	22.4	128.3	29.6	37.1	.755	.861	43.34	45.80	38.52
42	140.1	18.6	131.3	26.4	34.9	.703	.845	40.42	40.65	35.21

TESTS ON THE TRIPLE ENGINE AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY. — *Continued.*

INTERMEDIATE CYLINDER.

	Initial Pressure.	Per cent. Cut-off.	Pressure at Cut-off.	Pressure at Release.	Pressure at Compression.	Per cent. of Steam in the Cylinder at Cut-off.	Per cent. of Steam in the Cylinder at Release.	M. E. P. Crank.	M. E. P. Head.	Horse Power.
1	25	26	27	28	29	30	31	32	33	34
1	48.9	19.5	44 9	1.9	3.0	.829	.910	17.25	18.34	48.37
2	48.8	19.0	42.3	2.1	3.5	.798	.933	17.12	17.04	46.76
3	40.1	19.0	34.9	—0.6	1.7	.804	.927	13.91	14.95	40.10
4	35.1	22 0	28.8	—0.3	1.6	.797	.946	13.14	13.45	36.79
5	26.3	25.0	22.0	—1.7	0.4	.796	.931	11.15	10.72	30.52
6	22.1	28.0	17.4	—1.9	—0.9	.752	.892	10.98	10.69	27.79
7	20.4	29.0	15.8	—2.0	0.4	.821	.976	8.56	8.47	23.97
8	15.9	21.0	12 6	—5.9	—4.8	.719	.912	8.39	7.87	21.30
9	13 6	20.0	10 8	—6.3	—5.0	.758	.946	7.18	6 92	18.88
10	44 6	26.0	37.7	1.9	4.2	.847	.870	18.82	18.80	51.69
11	55.4	26.0	33.7	1.8	3.9	.852	.937	16.56	16.20	45.34
12	32 8	29.0	27.6	0.3	2.2	.866	.913	15.28	14.79	41.60
13	35.4	26.0	28.7	1.2	4.8	.852	.994	12.64	12.27	34.56
14	34 2	25.0	27 3	0.3	2.2	.841	.979	13.47	13.31	37.30
15	25.5	34.0	19.9	0.9	4.0	.901	Sup.	9.64	9.61	26.84
16	45.6	16.5	38.2	1.7	3.8	.640	.878	12.43	12.38	32.90
17	30.7	17.0	24.4	—1.8	0.4	.594	.853	9.51	8.75	24.53
18	40.4	21.3	33 8	—1.2	—0.2	.699	.735	16.38	16.52	44.18
19	38 1	19.0	32 3	—1.3	—2.1	.716	.818	17.23	16.54	45.99
20	32.2	22.0	27 3	—2.1	—1.0	.668	.746	13.16	14.11	36.63
21	32 3	19.3	25.9	—1.0	—0.2	.626	.849	10.96	11.45	29.94
22	36 4	15.5	29 6	—0.3	1.5	.579	.842	9.94	10.55	27.29
23	47 6	18.0	39.4	1.7	2.2	.667	.849	17.59	15.68	45.66
24	35.6	20.0	29.1	—0.9	—0.1	.645	.777	14.91	13.57	39.24
25	36.1	28.0	28.7	—0 6	—2.2	.841	.832	19.74	19 03	48.92
26	34.3	23.0	30.3	—1.7	—3.5	.832	.851	18.56	17.93	46.33
27	44 7	19.0	37.8	1.6	3.2	.726	.890	16.15	15.04	42 32
28	28.1	24 0	21.5	—2.2	—0.7	.759	.882	11.99	11.52	32.47
29	49 5	23.0	41.4	4.1	6.0	.789	.910	17.99	17.95	49.22
30	30.1	31.5	21.7	0.0	2.1	.834	.926	12.42	12.87	35.09
31	44.6	19 0	37.8	1.5	2.9	.740	.909	16.61	14.86	39.60
32	44 1	19.8	38 7	2.0	4.2	.768	.935	14.71	15.29	39.86
33	28 6	28.5	22.7	—0.1	1.1	.784	.912	12 28	13 46	34.50
34	20.7	28.0	15.6	—2.9	—1.9	.749	.873	10.54	10 46	27.04
35	41.0	21 0	34.6	0 2	0.3	.758	.853	17.17	16.46	45.15
36	55.3	20 0	48 6	4.7	5.8	.830	.962	19.54	19.77	53.75
37	36.5	26.0	30.4	1.0	1.8	.858	.948	15.39	15.38	42.50
38	17.4	32 0	13 6	—2.5	—0.5	.880	Sup.	8.11	7.85	20.76
39	18.2	29 0	14.9	—3.4	—0.3	.880	.982	7.54	7.74	19.98
40	52.8	18.0	45.0	1.3	0.0	.786	.883	20.79	19.44	55.08
41	27 5	28.0	20.4	—1.7	—0.3	.818	.920	12.51	11.79	33.97
42	26.8	27.0	21.5	—2.0	—0.9	.849	.924	12.22	11.62	33.51

TESTS ON THE TRIPLE ENGINE AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY. — *Concluded.*

	Low-Pressure Cylinder.												
	Initial Pressure.	Per cent. Cut-off.	Pressure at Cut-off.	Pressure at Release.	Pressure at Compression.	Per cent. of Steam in the Cylinder at Cut-off.	Per cent. of Steam in the Cylinder at Release.	M. E. P. Crank.	M. E. P. Head.	Horse Power.	B. T. U. per H. P. per minute (actual).	B. T. U. per H. P. per minute reduced to 26" vacuum.	Represented on plates (1 to 4) by
1	35	36	37	38	39	40	41	42	43	44	45	46	47
1	2.3	42.4	—1.1	—8.5	—11.2	.909	.861	8.52	8.66	53.01	240	233	
2	2.6	43.5	—2.0	—8.5	—11.2	.894	.895	8.30	8.43	52.01	241	237	
3	0.4	42.4	—3.1	—9.5	—11.7	.935	.880	7.28	7.55	46.78	237	231	
4	0.4	44.1	—3.5	—9.3	—11.9	.944	.921	7.37	7.44	46.52	239	236	
5	—0.3	41.5	—3.8	—9.9	—11.8	.947	.914	6.46	6.47	40.98	247	240	⊙
6	—1.7	53.0	—4.6	—9.7	—11.3	.996	.896	6.10	6.46	36.58	250	241	
7	—0.8	38.9	—4.3	—10.3	—12.1	.949	.911	6.52	6.61	41.98	255	255	
8	—5.0	47.0	—7.8	—11.2	—12.7	.996	Sup.	4.63	4.65	27.60	261	273	
9	—5.8	45.0	—7.8	—11.2	—12.1	.967	.991	3.83	3.89	23.19	274	274	
10	2.3	54.5	—1.3	—7.6	—11.5	Sup.	.951	9.05	9.68	58.82	249	244	
11	2.4	47.4	—0.9	—8.2	—11.7	Sup.	.965	8.87	9.26	57.07	244	240	
12	0.2	53.5	—2.7	—8.2	—11.8	Sup.	Sup.	7.95	8.12	50.83	246	243	
13	3.5	40.2	—1.2	—9.2	—11.8	Sup.	.931	8.68	8.66	54.64	242	237	⊡
14	1.1	46.3	—2.9	—9.3	—11.7	Sup.	.938	7.93	8.06	50.56	243	241	
15	2.6	38.0	—1.9	—8.6	—12.3	Sup.	Sup.	7.96	8.14	51.29	256	258	
16	2.5	21.0	—1.7	—10.4	—11.4	.518	.612	5.63	5.74	34.24	275	269	◈
17	—1.2	17.6	—4.3	—11.1	—12.2	.459	.624	4.01	4.03	24.54	285	280	
18	—1.0	32.2	—3.9	—9.6	—11.1	.613	.741	5.47	5.52	33.51	266	260	
19	—3.7	50.6	—6.2	—9.9	—11.2	.707	.756	4.99	5.20	31.52	263	257	△
20	—2.0	32.2	—4.7	—10.3	—11.3	.629	.736	4.72	5.03	29.73	298	264	
21	—0.3	44.9	—3.9	—9.5	—11.7	.760	.740	6.85	7.03	42.10	257	253	
22	1.2	42.0	—3.3	—9.1	—11.1	.741	.764	6.98	7.17	42.79	274	264	✱
23	0.8	43.0	—3.4	—8.6	—10.4	.720	.797	6.36	6.92	41.42	275	259	
24	—1.7	45.3	—4.6	—9.1	—10.4	.769	.775	5.60	5.89	35.97	281	271	✕
25	—3.6	53.0	—6.0	—9.5	—11.3	.731	.790	5.26	5.51	30.86	250	245	
26	—4.5	45.0	—6.2	—10.4	—11.7	.684	.726	4.72	4.96	27.92	252	252	✱
27	1.5	50.3	—2.1	—8.1	—10.8	.964	.908	8.40	8.59	52.37	255	247	
28	—1.7	53.2	—5.0	—9.6	—12.1	Sup.	.928	6.78	6.80	42.59	250	248	✲
29	5.0	43.0	0.6	—7.4	—11.4	.932	.915	9.95	9.91	61.75	255	248	
30	1.1	42.8	—2.4	—9.0	—11.8	.963	.915	7.79	7.89	49.39	249	245	✱
31	2.1	45.0	—2.0	—8.6	—11.2	.897	.890	7.57	8.55	46.09	258	256	
32	3.0	37.6	—1.0	—8.9	—12.0	.878	.852	7.56	8.59	51.71	254	252	
33	0.5	46.1	—3.2	—9.1	—11.5	.942	.909	7.30	7.36	44.61	249	242	✲
34	—2.9	50.2	—5.6	—9.9	—12.1	.835	.913	5.97	6.13	35.41	257	256	
35	—0.7	42.0	—4.5	—9.0	—10.9	.694	.832	5.74	6.07	36.01	248	234	✲
36	4.4	43.0	0.1	—7.6	—11.5	.927	.904	9.61	9.66	59.82	247	239	
37	1.4	44.3	—2.9	—8.9	—11.3	.914	.909	7.57	7.51	47.30	249	245	✲
38	—1.0	42.9	—4.5	—10.3	—11.6	Sup.	.984	6.32	6.58	38.11	261	262	
39	—1.4	39.3	—4.7	—10.8	—12.5	Sup.	.921	5.87	6.18	35.78	287	268	#
40	0.4	39.8	—3.3	—9.3	—10.9	.720	.773	6.36	6.75	40.78	277	268	
41	—1.3	30.0	—4.7	—10.5	—11.3	.665	.790	4.69	4.91	30.48	278	265	
42	—1.9	31.5	—5.4	—10.5	—11.9	.666	.792	5.06	5.26	32.95	269	265	✲

ENGLISH COMPOSITION IN COLLEGES.[1]

By GEORGE R. CARPENTER, Associate Professor of English.

Received December 12, 1892.

In noticing a recent treatise on rhetoric the *Nation* remarked that "a whole library of books on English composition would not teach a man to express himself on a subject beyond the range of his interest, his knowledge or his natural capacity. Hence the discipline surest of success is to be sought in narration and description, in making abstracts from good models, and in translation. The thinking may be left to take care of itself." Again, in a note on President Eliot's report of 1889–90, the same journal calls the especial attention of the reader to "what is said about the effort to improve the gift of expression in English at Harvard — a problem needlessly complicated, as we believe, by the endeavor to cultivate by one and the same exercise the art of clearly stating what a man has to tell, and to force the brain to think." What the *Nation* says may usually be taken as the result of thoughtful first-hand observation of some one qualified to observe and to judge. It is worth while, then, to examine these two statements a little further. In substance they agree, and they agree in recommending a kind of instruction which was more common a generation ago than now, and to which many a man of middle age will perhaps look back with misgivings or regret. The prevalent theory then was that rhetoric — the art of writing well — should be taught by a system in which precept played a larger part than practice, and in which practice was often subordinated to the academic study of so-called models of style. This theory was as a whole sound, for it was based on the well-tested and universally applicable principles of the Latin rhetoricians — abstract rules, smacking (to the taste of to-day) of Pope and Boileau, and well fortified with apposite illustrations from authors whose suave, dignified, and slightly pompous diction makes them appear to the humble reader impossible and unattainable ideals.

[1] Reprinted from the *Educational Review* of December, 1892.

Such a method and its results may be seen at its best in almost any Roman Catholic college, or in Du Cygne's excellent *Ars Rhetorica*, from which docile English-speaking pupils are still taught to respond (in answer to the question: "Quid confert Imitatio ad eloquentiam?") that "Imitatio confert ut optimo cuique Authori similes in dicendo evadamus, et quae sunt in eo summa, diligenter prosequamur." Nor were the results of the older method unsatisfactory when the pupils received seriously and practiced earnestly the instruction given them. If you compare a paper published by the undergraduates of a Jesuit college with the average journal published by the undergraduates of a college in which "modern" methods of instruction are in full vogue, the former may generally be found to excel in the essential qualities of style. The disadvantage of such methods is, however, that the pupil rarely conceives of rhetoric as having much to do with his other work in college.

Lack of interest on the part of the student — this is the chief fault of what we may call the "old" system. The subjects prescribed for him were alien to his natural sympathies, remote in time and interest, foreign to his current thought and feeling. He translated — to follow the scheme the reviewer indicates — from Pliny or Lessing, he made abstracts of Burke's speeches, he described the battlefield of Marathon or narrated the chief events of the siege of Syracuse. But all this, however excellent drill it may have been in the abstract, merely led the ordinary student to pretend an interest in that which was really distasteful to him; forced him to abhor such tasks as irrational, and to associate all composition with the conscious act of parading other people's ideas in other people's fine garments. Style then became for him something external, not woven of his own thought and emotion, but gained by imitation of accepted models. He must, he was taught, try to write with the brilliancy of Macaulay, or the swinging ponderosity of De Quincey, or the aphoristic unction of Matthew Arnold. But he must always make his bricks with the straw and clay which the taskmaster furnishes; he must never forage for himself, never give his own thoughts the expression that seems to him natural and appropriate to them, never be taught how, with a given subject in mind, to collect information from the best and most suggestive sources, never be urged to ponder over and assimilate what he has read and heard until it becomes thoroughly his, never be aided in the art of arranging his own material so that it will be most intelligible and effective, never, in

short, be helped to a man's birthright — that of mastering his own thought and conveying it by means of writing unchanged to the minds of others. Nor can we justly object, under such a system, to the idea that any subject-matter will serve the student's turn. Why should it not? Unless we "needlessly complicate the matter by helping the student to think" about what he writes, he may as well choose for his exercise "Balaam" or "The Advantages of a Cheerful Spirit" as something about which he has already been thinking for himself. His duty is not to concern himself with the subject-matter of his work save as he can apply to it those principles of composition or of style which are most in favor with his preceptor. And so the pupil passes through the required routine, frequently in such a machine-like fashion that he never dreams that the art he is learning is one which he uses and is to use almost as often as his legs, or that both the theoretical and the practical part of his work is scarcely valuable at all if it does not bring him to that clear, terse, and strong thinking which is at the basis of clear, terse, and strong writing. To the young man trained in this fashion the study called rhetoric soon becomes a dim memory. The writer recently had occasion to question some twenty or more students, who had wholly or partially completed their regular course of work in various colleges and scientific schools, as to what they had done in "Rhetoric." They had all studied it, in the early part of their college curriculum, for periods varying from one term to two years, but scarcely one of them could remember the name of the text-book over which he had spent so many hours. The color, the size, the shape, perhaps, lingered in his memory, but not the author's name or method.

In recent years, however, a far different method of teaching English composition has been followed to a greater or less degree in many of our colleges — at Harvard, perhaps, in particular. There, several processes of development have been steadily going on. First, on the rapid advance of the elective system has followed the slow retreat of all prescribed studies, including that of English composition, which is now reduced to a full course in the Freshman year, and to half a course in the two succeeding years. This prescribed drill is supposed to be sufficient for the needs of the average student. A number of elective courses, however, including both theory and practice, fully provide for the needs of those who wish to carry their studies further. Second, the character of the prescribed work has been changing no less steadily. In the Freshman year, to be sure, the drill is formal, and almost

entirely prefatory to really independent writing, but in the Sophomore year the class or kind of subject alone is indicated by the instructors, the subject itself being left to the choice of the student, who is encouraged by them to express in his written work his own more serious thoughts or fancies. In the Junior year, finally, the student chooses a subject of discussion from a list suggested by the instructors in various courses in philosophy, history, and science as fitting topics for investigation and thought. He is advised to write on subject-matter familiar to him through his other studies, to read on both sides of the question, and to let his thesis bear the marks not merely of clever phrasing or purely rhetorical skill but of thought, of judgment, and of logical power. Such is a fairly typical prescribed course in English composition in the college of to-day, differing widely — and it seems to me wisely — from that characteristic of the older method. In each case the groundwork is the same — a thorough theoretical training based on some particular treatise on rhetoric; but what follows is, according to the modern system, regulated by the principle that spontaneity is a prime requisite, that the pupil writes best and learns most when his subject-matter is to as great an extent as possible of his own choosing and prescribed by his own interests, although the transference of thought from one mind to another which rhetoric implies is in all cases one and the same effort, independent of the subject-matter which it concerns. Such a system proves itself successful by its success; the odium and irrational indifference with which an English department used frequently to be regarded has somewhat passed away, and teachers of rhetoric are beginning to be reckoned not with unnatural taskmasters but with other useful professors of other useful arts. We are, then, we may assume, on the right track in the main. But are there no further modifications of the system now in vogue that will make our teaching still more effective? With the same expenditure of force — to wit, with the same number of instructors, with the same working hours on the part of both student and lecturer (or theme-corrector), with the same mental wear and tear on both sides, can we not arrive at results even more satisfactory?

For, as a matter of fact, the English departments in our colleges, in spite of the rapid progress made of late years, are in many cases scarcely doing half the work that might on *a priori* grounds be expected from efficient organizations. At the outset the work is handicapped. Young men come to college at an average age of eighteen

or nineteen, and frequently with more maturity of character than their ages would indicate. But in nine cases out of ten they come badly trained in English. This lack of training shows itself in two forms: inability — coming from lack of knowledge — to avoid plain grammatical errors; and inability — coming from lack of practice — to begin the work of writing in anything resembling a simple and businesslike manner. For half a year lecturing instructors and correcting instructors are obliged to spend the greater part of their time in discussing, in general and in particular, plain blunders of style which a boy of sixteen should have done with once for all; in elaborately explaining such fundamental principles as should have been familiar to the same boy as soon as the axioms of geometry; and in what seems for months a fruitless attempt to bring the class up to a general level of information and intelligence that will enable it to attack successfully the real problems which face the young writer.

Again, the old indifference still remains. In most colleges, and especially in scientific schools, the bulk of a class is indifferent to the study of rhetoric as now taught, escapes and evades it, and comes out of it scarcely better than when it went in. The fact is that everybody — students under the charge of the department and critics outside — appreciates the artificial character of the present system. For artificial it is in essence as long as men write for the mere sake of writing, with no distinct desire to communicate thought. The stronger men, therefore — the obviously necessary drill of the Freshman year over — neglect routine work in composition on the ground, maintained often and in so many words by wiser heads than theirs, that when they have something to express there will be time enough to think about the various ways of expressing it. Weaker men, on the contrary, learn to regard English work as a sort of foppish literary self-adornment. The majority of the class go meanwhile heedlessly on their way, writing themes by the recipes which are given them by the instructor or which they find current among their friends, in very much the same way that the careless schoolboy applies the same formula to example after example in a branch of mathematics which he neither enjoys, understands, nor sees the importance of. Instructors, on the contrary, mostly earnest men, work seriously at criticising themes into which the writers have never dreamed of putting serious work. Candidates for the degree of B. A. or B. S., as a result, in theses, in reports, in their private or public letters, often write in a slovenly, haphazard

fashion which is the despair of all who think it necessary that any man old enough to vote should be able to express such matters as he may have to treat in writing accurately, clearly, and forcibly.

The plain fact is, it seems to me, that departments of rhetoric still neglect an important factor. The difficulty of getting boys well trained in habits of expression before they enter college we can leave to be settled by the growing movement which is bringing up the preparatory school to a proper level of efficiency. But the point often overlooked is that no man or boy can be made to write really well unless he writes for the purpose of expressing thought. It is possible, indeed, that he may be seized with a desire to express ideas of his own to his preceptor in rhetoric in the routine themes demanded of him, but that is unlikely. Writing for his instructor, and for him alone, he is tempted beyond his power of resistance to mold his thought to suit his expression, not his expression to suit his thought. But every student is continually writing, unconscious of rhetorical rules or with only a confused idea of them in his memory, without particular instruction, but under the most appropriate conditions, matter which in many cases is just what his instructor tries in vain to secure from him in his routine work. What could be better drill in composition than these reports, theses, and abstracts, prepared for his instructors in history, philosophy, or science? Given, for instance, various lectures or other sources of theoretical or practical information, what, the student is asked to report, is the effect of algæ growths in reservoirs and ponds, or what is to be said of pin bridges *versus* riveted bridges? Here the sound man comes to the front, not the turner of meaningless and quibbling phrases. The student who, in set work, in routine themes, broke every law of sense and taste in composition, now writes — clumsily, to be sure, but with his eye on what he wants to express and a steady effort to attain it. The man who thought punctuation a useless and finicky operation finds that he must master it if he wants to make perfectly intelligible what he has to say on metallurgical methods in the mines of Bolivia. The man who disdained the study of emphasis, climax, unity, or what not, finds it indispensable if he wants to make telling points in his plea for the abolition of grade crossings. Paragraph structure instantly becomes luminous, and even sentence structure, if the teacher is skillful in following out the connection between thought and expression. The clever artificiality vanishes, and industry, sense, and sound thinking come to the front.

Such a system may seem identical with that mentioned as already in use at Harvard — the so-called forensic system. There is, however, a decided distinction between them. In the first the student still writes for the English instructor; in the system which I propose, with much diffidence but on the basis of actual experiment, the student, after the necessary drill of the Freshman or sub-Freshman year, never writes expressly for his English instructor, but always for the department to which by subject-matter his thesis or report actually belongs. Subjected to criticism from both sides, on the basis of thought and that of expression, by both departments working together for their common good, the thesis or theme is thus treated as it should be. It would be surprising to see how nearly identical in purpose and in effect the criticism given to the student by the specialist in the department to which his essay belongs by right of subject-matter is with that given him by his instructor in rhetoric were it not obvious that the best writers in the faculty of any college, university, or even scientific school are rarely those who represent the department of rhetoric. How, indeed, could it be otherwise? Expression implies thought, and he who thinks most clearly, definitely, forcibly, and broadly is likely, unless he has great defects in training or taste, to write better than he who, however much he is interested in the so-called art of expression, has too frequently narrowed his interests and perhaps even debauched his intellect by confining himself for years to the incessant reading of themes merely made to order by tired, lazy, or indifferent students.

Of course any large college would encounter many and perhaps great difficulties in adopting such a system and in giving it an organic structure. But such experiments as I have been able to carry on in the Massachusetts Institute of Technology, with students preparing for the technical professions, and what I have seen of the success of the somewhat similar "forensic" system at Harvard, convince me that the reports and theses which most teachers of rhetoric have for years been passing by as unworthy of notice may be made of the greatest value in teaching college boys to write. Good elementary training, then, is what we need in the Freshman year; after that as many elective courses as seems advisable; but for the average man in his Sophomore, Junior, and Senior years, frequent practice in writing about what he really knows, under the criticism not only of the instructors whose sole object is to teach him to write, but also of those who, in whatever way, lead him toward knowledge or help him to think for himself.

JAMES BICHENO FRANCIS.

By HIRAM F. MILLS.

Read to the Corporation of the Institute of Technology, December 14, 1892.

Our friend, and the friend of this institution, whose name has for five years stood first in the list of members of this corporation, passed on to his new field of usefulness on the 18th of last September. From its organization for twenty-seven years Mr. James B. Francis has attended the meetings of this board, worked on its committees, advised and encouraged the Faculty of the Institute, and has been a personal and professional aid to many of its graduates.

Born at Southleigh, Oxfordshire, England, on May 18, 1815, Mr. Francis, like his loved friend, Mr. Hoadley — whose memory we all revere — began the work which led on to his profession at the early age of fourteen. Mr. Hoadley at that age began work in a machine shop, and Mr. Francis began assisting the engineer in constructing harbor works in Porth Cawl in South Wales, of which his father was superintendent. Two years later, in 1831, he was employed on the construction of the Grand Western Canal in Devonshire and Somersetshire. In the spring of 1833, before he was eighteen years old, he emigrated to this country, and soon found employment as an assistant engineer under William Gibbs McNeil and George W. Whistler, on the New York, Providence and Boston Railroad.

In 1834 Mr. Francis went with Major Whistler to Lowell, and began his work there by making working drawings of the details of one of the locomotives built by Stephenson for the Boston and Lowell Railroad, for reproduction in the machine shop of the Proprietors of Locks and Canals on Merrimack River. He remained as assistant engineer under Major Whistler until 1837, when the latter left Lowell and soon after went to Russia to introduce railway construction in that country, and Mr. Francis, at the age of twenty-two, was made chief engineer of the Proprietors of Locks and Canals on Merrimack River, which position he held for forty-eight years.

In 1845 he was invited by Mr. Storrow to assist him in developing the water power of the Merrimack River at what is now Lawrence,

but the Proprietors of Locks and Canals concluded that they needed him more at Lowell; they consequently increased his salary, and on September 27, 1845, appointed him their agent and engineer, and for the following forty years he had the management of the water power there, and the construction of the numerous improvements that have been made in the works connected with it. His duties also included the position, without name, of consulting engineer to the various manufacturing corporations at Lowell. Seeing that the performance of these duties in a manner satisfactory to him required greater preliminary training in mathematics and applied mechanics than his early education had afforded, he applied himself assiduously to the study of these subjects, taking by himself, in the leisure hours of a very busy life, an advanced course of study in these subjects, which put him abreast of the best educated engineers of his generation.

His habits of thought were methodical. He was often asked by the directors of his company, or by the agents of the manufacturing companies, in regard to questions concerning their peculiar departments of business, in such variety that no one not in the special business could be expected to give advice; but he, taking the question definitely in mind, went to his office and began a page of his calculation book with "Mr. —— wants to know" so and so. He would then write out in full whatever of value he could find published upon that subject, together with his conclusions, and when he met his questioner again was ready to discuss the subject in all its bearings and give intelligent advice. From this habit of thoroughly investigating the subjects that came before him, and writing out his data and conclusions in form for convenient future reference, he came to be rightly acknowledged as an authority on a great variety of subjects.

The experiments of Eaton Hodgkinson on the best form for cast-iron beams had been published three years previously, but before using the results in an important position Mr. Francis preferred to try beams made of the full size required, and of iron commonly used for such purposes in Lowell. He accordingly had a beam constructed having a span of nineteen feet and a depth of fifteen and one eighth inches, and on January 7, 1845, broke it with 69,821 pounds applied at the center. He then altered the pattern and broke another beam with a load of 114,000 pounds. These are the earliest experiments I have found upon large beams of American iron.

In 1846 Mr. Francis made designs for the enlargement of the water power at Lowell, and constructed the grand canal with its river

wall thirty-six feet in height, the body of which is a dry stone wall with joints perpendicular to the battering face, and having a water tight lining of small stones in cement on the canal side. He here introduced vertical screws for hoisting the head gates with nuts driven by a turbine. The entire cost of these improvements, amounting to $530,000, very slightly exceeded his estimates. In constructing the guard dam at the head gates of this canal, in 1847, he determined the proper height by investigating the past great freshets that had occurred on the river, and concluded that the guard dam and lock at the old canal required to be raised. They were built to the height of the new guard dam in 1848. Then Mr. Francis concluded that the lock gates were insufficient to withstand such a freshet as occurred in 1785, and recommended the construction of a great gate twenty-seven feet wide and twenty-five feet high. This was accordingly built in 1850, and hung high in the air over the lock. The need of this gate was beyond the comprehension of most of the people, and it was not uncommonly spoken of as Francis' folly. Only two years later, in April, 1852, a freshet arose higher than ever before above the dam until there was imminent danger of destroying an immense amount of property.

Mr. Francis, in writing of the great freshets in the Merrimack River, modestly says: "The great gate in the guard lock was hung in 1850, two years only before the freshet of 1852, when it was dropped. This is the only occasion when it has been of service, but it is kept in readiness for use at any time. The new gate was placed across the lock about forty-six feet below the old gates, which were not changed, but were supposed to be sufficient to hold until the water attained the height it was in 1785, or to about thirteen and a half feet above the top of the dam; but it proved to be otherwise. At three o'clock in the morning of April 22, 1852, the water found its way round the heel post of the westerly gate, the height of water at the time being eleven feet ten and a quarter inches above the top of the dam. Preparations were immediately made to drop the new gate, which was done half an hour afterwards. The water continued to rise until one o'clock the next morning, rising in the interval two feet two and three quarters inches, or to the height of fourteen feet one inch above the top of the dam, which was the maximum height attained in this freshet, or indeed of any other of which we have any record or tradition."

The day after the gate was dropped, but before the water began to recede, the Lowell *Journal* said: "Lowell has probably escaped a great calamity by the wise foresight and timely precaution of one eminently practical man — James B. Francis, Esq., Agent of the Proprietors of Locks and Canals on Merrimack River. Mr. Francis, a few years since, anticipating the possibility of an event such as we have just realized, and foreseeing that the strong works at the 'Guard Locks' might fail in such an emergency, caused to be constructed a massive, higher and stronger gate, which has ever since been kept in a position to be lowered to its place at a moment's notice. Early yesterday morning it was clearly evident that the safety of the Company's works, and consequently of a goodly portion of the city, required the lowering of the huge gate to its post of duty. It was promptly done, and danger from that source was no longer within the range of possibilities."

The Boston *Daily Advertiser* a few days later said: "It is certainly somewhat remarkable that a freshet occurring only once in a century should happen only two years after special preparation had been made for it, and the inhabitants of Lowell, as well as the proprietors of the mills, have great cause to thank the gentlemen who so promptly decided to take measures to guard against what every one considered a very remote contingency, and what many considered a useless expenditure of money.

"It is awful to think of what would have been the inevitable result if the new works had not been constructed. Every vestige of the old guard gates would have been carried away, and a mighty and uncontrollable river would have swept through the heart of Lowell, destroying everything in its course."

A few years later citizens of Lowell presented to Mr. Francis a service of silver "in token of their admiration of his foresight and sagacity in constructing a gate at the Guard Locks, thereby effectually protecting the city of Lowell against those casualties to which, as the freshet of 1852 fully demonstrated, it must be exposed."

In 1849 Mr. Francis was sent to England by the manufacturing companies of Lowell to observe the methods in use there for preserving timber from decay, which resulted in the construction of works at Lowell for kyanizing and for burnettizing timber with experiments upon wood so treated, some of the results of which are published in the transactions of the American Society of Civil Engineers.

Among other notable engineering works at Lowell are his ingenious and skillful application of hydraulic lifts to the guard gates of the Pawtucket Canal, designed in 1870, and the reconstruction of the Pawtucket Dam across the Merrimack River in 1875–76.

A prominent part of Mr. Francis' duty at Lowell was to distribute the water power among the several corporations in accordance with their respective rights; this has called for the execution of many original hydraulic experiments on a large scale. Selections from those made from 1847 to 1852 on hydraulic motors and on the flow of water over weirs and in short canals of uniform rectangular section were published in 1855, in the work entitled *Lowell Hydraulic Experiments.* In the second edition, published in 1868, were extensive additions upon the subject of the flow of water in open canals from experiments made from 1856 to 1860, and very interesting results of experiments made in 1854 upon the flow of water through submerged orifices and diverging tubes. The third and fourth editions were issued in 1871 and 1883.

This work forms an era in the engineering literature of America as well as in the growth of the profession. The extreme accuracy of the work and the care with which all interfering circumstances were avoided and the clearness and fullness with which all of the actual conditions are presented give it a character which younger engineers have been proud to emulate, and it has had a remarkably great influence in the growth of the engineering profession in America.

Mr. Francis was elected Fellow of the American Academy of Arts and Sciences, November 13, 1844. On February 14, 1865, his memoir, *On the Strength of Cast-Iron Pillars with tables for the use of Engineers, Architects and Builders*, was presented to the academy. Prof. Daniel Treadwell, addressing the academy, said: "Mr. Francis has made a most thorough examination of all the experiments that have been published, or that came within his reach, with the conclusions which have been drawn from them, together with the formulas and rules that have been proposed for practical constructions. Proceeding thus he has, after a careful and patient examination of the experiments and the reasoning of the several investigations, selected such of the conclusions and formulas as have appeared to him quite trustworthy, and from combining and in some cases modifying these he has calculated a most elaborate set of tables, by which the architect or practical workman can find a column suited to any structure, and

which may be relied upon as safe for all purposes, ordinary, or even extraordinary. I consider the paper thus produced by Mr. Francis as of great value, and believe that it will become a work of the first authority and of constant reference for the builder."

This paper was worked out to supply a need in the profession shown by the fall of the Pemberton Mill in Lawrence, January 10, 1860, an account of which was published by Mr. Francis in the Journal of the Franklin Institute immediately after.

In the same journal may be found important papers by Mr. Francis: in 1867 on experiments on the evaporation of steam boilers used for heating purposes, a description of cofferdam at Turner's Falls and formulas and tables for the shafting of mills and factories; in 1870, experiments on the evaporation of a Corliss boiler; and in 1875, report on a test trial of a Swain turbine water wheel.

Mr. Francis was elected a member of the American Society of Civil Engineers at its first meeting, November 5, 1852, and was president of the society from November 3, 1880, to January 18, 1882, and on the 5th of April, 1892, was elected an honorary member. To this society he contributed many valuable papers, and joined in many of the discussions. Among the papers containing additions to the knowledge of the profession are his "Experiments on the Deflection of Continuous Beams," "Experiments on the Flow of Water over Submerged Weirs," "Experiments on the Preservation of Timber," and "Experiments on the Percolation of Water through Portland Cement Mortar," contained in his paper on "High Walls or Dams to Resist the Pressure of Water." He also contributed reports on the failure of the dam on Mill River, and on the cause of the failure of the South Fork Dam; also papers on durability of cast-iron water mains, the Provincetown dike, translation of Darcy's results on flow of water through cast-iron pipes; on the cause of the maximum velocity of water flowing in open channels being below the surface; distribution of rainfall over New England during the great storm of October 3 and 4, 1869; experiments on the Humphrey turbine water wheel; stoppage of flow in a water main by anchor ice; and on the effect of a rapidly increasing supply of water to a stream on the flow below the point of supply. And in his address as president of the society, delivered at Montreal in 1881, he gave the result of his observations on the formation of anchor ice.

Mr. Francis became a member of the Boston Society of Civil Engineers July 3, 1848, and was its president in 1874. To the meet-

ings of these societies of engineers he was always heartily welcomed, and was listened to with profound respect. His character as an investigator seeking only the truth, as a skillful engineer adopting the best means to accomplish the end desired, and as a pure-minded, honorable man, impressed his associates; and with the greatest confidence in his integrity there was added the enjoyment of his geniality and helpful kindness.

He received from Dartmouth College, in 1851, the honorary degree of Master of Arts, and in 1858 the same degree was conferred by Harvard College. He was a member of the American Philosophical Society of Philadelphia, and from 1864 to 1868 a member of the Boston Society of Natural History. He was also honorary member of Winchester Historical and Genealogical Society, of the Arkwright Club, of the Trinity Historical Society of Dallas, Texas, and of the American Society of Irrigation Engineers of Salt Lake City, Utah.

Mr. Francis has had a wide practice throughout the country as consulting engineer in hydraulic work, in which department he has been regarded as the head of his profession in this country. Among the more important subjects on which he has given advice are fifty water powers in nine States and two provinces, including a plan for preventing the recession of St. Anthony's Falls on the Mississippi River, and designs for reconstruction of the Holyoke Dam; also water works in eighteen cities of five States and one province, including a report on the design for the Quaker Bridge Dam on Croton River; also many foundations of important structures, including that of Trinity Church tower in Boston; also upon irrigation in California. He has been employed as an expert in a great number of cases at law.

Mr. Francis resigned the office of agent and engineer of the Proprietors of Locks and Canals on Merrimack River, January 1, 1885, which he had held from September 27, 1845. This was fifty years after he entered the service of the company, and his friends — the second generation of directors during his service — then presented to him a beautiful service of silver, and to their expressions of trust and friendship added the declaration, "To the eminent ability and wisdom which have distinguished your administration, the marked success of the Lowell manufacturers has been largely due." He was then appointed consulting engineer of the company, which position he held at the time of his death, September 18, 1892.

Outside of his profession he was held in the highest esteem by the community for his sterling integrity and kindliness. Everybody had

the utmost confidence in his judgment, in his fairness, in his unselfishness and in his great executive ability. He served one year in the State Legislature, five years in the city government, four years on the school committee, two years as director of the city library, one year as president of Middlesex Mechanics Association, twenty years as president of Stony Brook Railroad, thirty-two years as director of the Railroad National Bank, and forty-three years as director of the Lowell Gas Light Company.

He was frequently appointed a commissioner to decide important cases, in which his professional knowledge and his judicial mind would give advantages over others. Among the more important of these commissions were those to assess damages for the diversion of Sudbury River by the city of Boston, in 1876, and to report on method of preventing floods in the valley of Stony Brook, in 1886; also to design and construct for the State of Massachusetts the Provincetown Dike, in 1867–69.

By the will of his friend, the eminent engineer Uriah A. Boyden, Mr. Francis was made one of the trustees of a fund, amounting to two hundred and thirty thousand dollars, to establish an observatory, or to help others to maintain an observatory for astronomical research "at such an elevation as to be free, so far as practicable, from the impediments to accurate observations which occur in the observatories now existing, owing to atmospheric influences." To determine where best to expend this money Mr. Francis visited, principally at his own expense, nearly if not quite all of the observatories of this country. The trustees finally transferred the property to the president and fellows of Harvard College, to be used for the purpose intended with other funds of the observatory, resulting in the establishment of an observatory near Arequipa in Peru, at an altitude of more than eight thousand feet above the sea.

With the unsurpassed activities of his wonderfully useful life, in which he illustrated the noble Christian virtues, he yet had time and thought and activity to devote to the interests of St. Anne's Church, of which he was member, warden and vestryman, and an interested director of their orphanage.

In all the relations of life he was the faithful, single-minded, courageous, reliable, earnest, far-sighted, honorable man. His memory is a blessing to his country.

Lawrence, December 11, 1892.

BOOKS RECEIVED.

Endlich, F. M. Manual of qualitative blowpipe analysis and determinative mineralogy. xv, 456 p. il. 1 pl. O. N. Y., Scientific Publishing Co., 1892. cl. $4.00.

Hofman, H. C. Metallurgy of lead and the desilverization of base bullion. 414 p. 275 il. O. N. Y., Scientific Publishing Co., 1892. cl. $6.00.

Noyes, Arthur A. Notes on qualitative analysis. Prepared for the use of the classes of the Massachusetts Institute of Technology. 58 p. O. Boston, Author, 1892. cl.

TECHNOLOGY QUARTERLY

AND

PROCEEDINGS OF THE SOCIETY OF ARTS.

VOL. V. DECEMBER, 1892. No. 4.

PROCEEDINGS OF THE SOCIETY OF ARTS.

THIRTY-FIRST YEAR, 1892–93.

THURSDAY, November 10, 1892.

THE 436th meeting of the SOCIETY OF ARTS was held at the Institute this day at 8 P.M., President Walker in the chair.

The records of the previous meeting were read and approved. The President then introduced Lieut. H. L. Hawthorne, Fourth United States Artillery, who read a paper on the "New Tactics." The paper is published in full in the present number of the *Quarterly*. After the reading of the paper the President extended the thanks of the Society to Lieutenant Hawthorne, and declared the meeting adjourned.

TUESDAY, November 22, 1892.

The 437th meeting of the SOCIETY OF ARTS was held at the Institute this day at 8 P.M., President Walker in the chair.

The records of the previous meeting were read and approved. The President then introduced Prof. T. C. Mendenhall, Superintendent of the United States Coast Survey, who read a paper on the "Standards of Length and Weight of the United States." After some discussion the President extended the thanks of the Society to Professor Mendenhall for his very interesting paper, and declared the meeting adjourned.

THURSDAY, December 8, 1892.

The 438th meeting of the SOCIETY OF ARTS was held at the Institute, in the Walker Building, this day at 8 P.M., Mr. George W. Blodgett in the chair.

The records of the previous meeting were read and approved. Dr. Henry P. Talbot, of Boston, and Mr. J. W. Smith, of the Institute, were duly elected Associate Members of the Society. The Society voted to adopt the recommendation of the Executive Committee to assume the assets, including plates and back numbers, and the liabilities of the past year (Vol. IV) of the *Quarterly*.

The following paper was read by title: "Upon the Difficulty with which Water Parts with its Dissolved Oxygen," by Augustus H. Gill, Ph.D.

The Chairman then introduced Mr. I. H. Farnham, of Boston, who read a paper on "Lightning and Strong Current Arresters for Telephones." It is published in full in the present number of the *Quarterly*. The paper was illustrated by diagrams, and at its close Mr. Farnham showed experimentally the efficiency of the different devices. The meeting then adjourned.

THURSDAY, December 22, 1892.

The 439th meeting of the SOCIETY OF ARTS was held at the Institute, in the Walker Building, this day at 8 P.M., Mr. H. M. Howe in the chair.

The records of the previous meeting were read and approved. Dr. A. H. Gill, of the Institute, was duly elected an Associate Member of the Society. The following papers were read by title:

"Tests on the Triple Engine at the Massachusetts Institute of Technology," by C. H. Peabody and E. F. Miller.

"English Composition in Colleges," by George R. Carpenter, Associate Professor of English.

The Chairman then introduced Mr. George D. Burton, President of the Electrical Forging Co., who read a paper on the "Heating and Working of Metals by Electricity." The paper was illustrated with the lantern. It is published in full in the present number of the *Quarterly*. After a short discussion the thanks of the Society were extended to Mr. Burton, and the meeting was adjourned.

CLEMENT W. ANDREWS, *Secretary*.

THE CHANGES OF THE YEAR.

By FRANCIS A. WALKER, LL.D., President of the Institute.

The changes of the year at the Institute have been numerous and important. Unfortunately they have been in the way of loss as well as of gain, two highly valued members of the faculty having gone from us — one by death and one by resignation.

Let us consider first the material changes of the year. The department of Architecture, having outgrown its rooms in the Walker building, has been provided with a building of its own. It may be interesting to pause a moment to refer to the successive homes which this department has occupied during the past eleven years. When I came to the presidency of the Institute, in 1881, the "architects" occupied one half of the room now devoted to free-hand drawing, in the lantern story of the Rogers building. When the Walker building was erected, in 1883, one half of the second story was assigned to this department. When, in 1889, the civil engineers removed to the new Engineering building the Architectural department expanded to fill the whole of the second story, except Professor Cross' lecture room. They have now, as stated, a building all of their own. The front of this building is a continuation of the front of the Engineering building, extending 68 feet southward along Trinity Place towards the Providence railroad. The building contains five stories and a basement. Three of the stories form single, large drawing-rooms, 58 feet in depth, the uppermost being devoted to free-hand drawing, water color, and life-class work. This room is lighted to the point of perfection, having extensive skylights which allow almost every chair that can be placed in the room to be occupied for artistic work to the highest possible advantage. The third story is parted into a drawing-room (for the Fourth Year class) and a library, sumptuously furnished, which not only provides shelves and cases for the one thousand books and the ten thousand photographs belonging to the department, but also a wide spread of tables on which these can be laid out and studied with the

maximum of convenience. The first floor is divided into three lecture and recitation rooms, the largest being equipped with the lime-light. The basement is equally divided into a museum of building appliances and a rough laboratory for clay modeling, for mixing mortars and cements, and for other experimental work. The inner walls of the Architectural building are hung with thesis and other drawings of former students. Here, too, are found the superb architectural drawings of Emanuel Brune and the rich and beautiful *envois* of the Rotch traveling scholars. To say that the architects are happy in their new home would be, as the boys say, "no word for it." The work of the year has been carried on with a zest and delight which clearly indicate how much the Institute might gain could all its departments and services be amply and conveniently provided for.

The removal of the Architectural department has allowed great changes in the Walker building to be effected, to the marked relief of the departments of chemistry, physics, and modern languages. To the last has been assigned an excellent suite of recitation rooms in the second story, with a common room, adjacent, for the use of the five instructors. The Chemical department has been given three additional laboratories on the third floor, two of these being 44 by 30 feet in area. This additional space has allowed a very considerable rearrangement of the chemical work of the Institute, as well as a large extension in the direction of research and thesis work. The Kidder Chemical Laboratories now comprise eighteen laboratories, four lecture rooms, a reading room and library, and a balance room, with offices and supply rooms, making in all thirty rooms, affording ample accommodation for six hundred and twenty-five students in chemistry. But it is, perhaps, the Physical department which has profited most largely by the change. The additional space assigned to Professor Cross has enabled him to extend his two main laboratories each 25 feet toward the north; to fit up a new apparatus room and a large and commodious reading room and library for his department; and to organize in the second story a suite of three laboratories for special research — one in acoustics, one in optics, and one in electricity. Numerous other minor changes in the several buildings of the Institute were made with reference to the work of the current year, with which I will not weary the reader.

The most important of the additions to the apparatus and the machinery of the Institute has been the 300,000 lbs. Emery Testing

Machine, recently set up in the basement of the Engineering building. This great engine was expected to be in place last February ; but the delays incident to its size and to the fact of its being the first constructed by Messrs. Sellers & Co., of Philadelphia, prevented it from being available before the opening of the second half of the current year. The new testing machine has already proved itself a most valuable addition to the resources of the school, having a delicacy fully corresponding to its great power. A new Universal Milling Machine has been added to the equipment of the shops, which still remain, as they have long been, the largest and best furnished laboratories for instruction in the mechanic arts to be found in the world. Extensive additions have also been made, from the designs of Professor Porter, to our unequaled laboratory of Hydraulic Engineering, enabling that department to carry on to the best effect every species of hydraulic experiment which can be conducted within walls. Extensive additions have also been made during the year to all the scientific and technical libraries. No fewer than 362 periodicals, including all scientific and technical journals and magazines which any department deems it important or even desirable to have, are now regularly taken at the Institute. The approaching completion of the new Boston Public Library in our immediate neighborhood will further add greatly to the resources of the school.

But the greatest of the changes in the material conditions of the Institute of Technology which have taken place during the last twelve months has been through the acquisition of land. As I write, the Institute has just acquired the skating-rink property on Clarendon Street, and the Jordan land lying between the Institute property on Trinity Place and the Providence railroad. These two estates aggregate about 50,000 square feet ; the sum paid for them was in the neighborhood of $275,000. So great is the poverty of the Institute that it has been necessary to raise nearly the entire amount by mortgage of the properties so purchased and of the Architectural and Engineering buildings. To the old-fashioned college trustee such a transaction would seem little short of madness ; but the greatness of the Institute of Technology is mainly due to the fact that from the first its destinies have been controlled by large-minded business men who dared to take risks, to bank upon the future, and to trust to the munificence of the citizens of Boston and Massachusetts. Ten years ago we ran in debt $300,000 for the erection of the Walker building

and the Workshops and for the purchase of the land on Garrison Street. To-day that debt is paid; the Institute is three times as large as it then was; its work is better done in every department; and its reputation and influence have been greatly extended. In purchasing the new land the trustees of 1893 have recognized the fact that a new era in the life of the school has opened, and that the new exigencies and necessities must be met with the same courage and comprehension which characterized the action of their predecessors, or else the Institute must fall short of its high mission and fail of some part of the duty which the country demands of it.

Let us turn now to the changes in the *personnel* of the Institute of Technology the past year. In Mr. James B. Francis, of Lowell, the Corporation has lost one of its charter members, one of its firmest and most devoted friends, the greatest of American hydraulic engineers, and one of the foremost engineers of the age. In another article of this volume of the *Quarterly* will be found the just, feeling, and discriminating eulogy upon Mr. Francis read at the last meeting of the Corporation by his worthy colleague and lifelong friend, Mr. Hiram F. Mills. In Mr. M. Denman Ross the Corporation has lost another of its charter members, one of the projectors of the Back Bay district of Boston, and an early and persistent advocate of the scheme of founding in that district a great school of arts and sciences.

The faculty, likewise, has lost two of its members during the year. Professor Luquiens has resigned the professorship of modern languages to accept the professorship of the Romance languages at Yale — a position in which his superb scholarship will find ampler opportunities than were afforded it by the elementary instruction which he had for eighteen years conducted at the Institute of Technology with such marked success. Professor Luquiens carries to his new post of duty the cordial affection and respect of a host of "Institute men."

On the 28th of November, 1892, Professor Eugène Létang, for twenty-one years in charge of Architectural Design at the Institute, died after a brief illness, to the great loss of the school and to the inexpressible regret of all his former associates and pupils. During the long term of his service here, Professor Létang has never ceased to exert a profound influence upon the study and practice of architecture in the United States, and of few men can it be so truly said that his influence will survive him. Hundreds of his pupils are now leading architectural practice in all parts of the United States under the

inspiration they derived from him during their school life here, and the impulse which he has given to sincere, pure, and high-minded architecture in this new country will be accelerated rather than retarded with the passage of years.

The problem of filling Professor Létang's place was necessarily one of very great difficulty. His work for the present year has been taken in charge by Mr. Samuel W. Mead, of the firm of Cabot, Everett & Mead. Mr. Mead, one of the earliest recipients of the Rotch traveling scholarship and an artist who is in complete sympathy with the thoughtful, thorough, and academic spirit which characterized the drawing-room work under Professor Létang, has applied himself with the highest zeal and intelligence to prevent any falling off in the Design of the year, while Professor Chandler and his assistants have thrown themselves into the breach to repair as far as possible the loss which has been sustained. Meanwhile the corporation have felt that it was their duty to go back again to that highest source of modern inspiration in art from which Professor Létang came directly to us. In searching throughout France among the recent graduates of the École des Beaux Arts for that one who should be most worthy to succeed our departed associate, we have received valuable advice and assistance from Mr. Arthur W. Wheelwright and Mr. J. Randolph Coolidge, Jr. It is with great gratification that I now announce, through the pages of the *Technology Quarterly*, a result more fortunate than we had presumed to expect. Monsieur D. Despradelle, one of the most distinguished of recent graduates of the Beaux Arts, and an architect already of assured position in his own country, has accepted the associate professorship of architectural design. M. Despradelle graduated with much distinction at the unusually early age of twenty-five. He had taken most of the minor prizes of his time, and in the competition for the Prix de Rome won the rank of Premier Second. M. Despradelle is now *Sous Inspecteur aux travaux des Bâtiments civils des Monuments de l'État.*

Still another death has occurred among the corps of instructors since I last wrote for the *Quarterly* on the "Changes of the Year." In August last Mr. George V. McLauthlin, instructor in biology, was drowned off the shore while bathing at Nahant. Mr. McLauthlin graduated in chemistry with the class of 1888. Before his untimely death he had already attained a high position both as teacher and as investigator, and bade fair to become one of the most distinguished

graduates of the Institute. Other losses to the corps of instructors have been through the resignation of Dr. Gardiner and Messrs. Williston, Cobb, Heller, and E. V. French. The corps of instructors has been reënforced the present year by the appointment of Messrs. Ch. L. N. Bernard, Joseph Blachstein, and Johann Meyer, in modern languages; Messrs. Harry M. Goodwin and Wm. L. Smith, in physics; Mr. Richard E. Edes, in biology; Mr. Edward Robinson, in mechanical drawing; Messrs. Leonard M. Passano and Wm. H. Metzler, in mathematics.

The changes in the faculty other than those mentioned have been through the promotion of Associate Professors Dewey and Levermore to be Professors of Economics and Statistics and of History, respectively, and of Assistant Professor H. W. Tyler to be Associate Professor of Mathematics; through the appointment of instructors Henry P. Talbot, Dana P. Bartlett, Edward F. Miller, and Frank Vogel to be Assistant Professors of Analytic Chemistry, Mathematics, Steam Engineering, and Modern Languages, respectively; and, lastly, through the appointment, for the first time in fourteen years, of a military instructor with faculty rank. The last mentioned appointment is one of so much consequence to the school that I will speak of it more at length. Ever since the foundation of the Institute instruction in military tactics has been given in the school, as provided by the act of Congress of July 2, 1862, under which we receive a portion of our annual income. The requirement of military instruction has always been honestly dealt with here — a substantial course being given, without interruption, to the students of the first year. During the summer of 1891 the Executive Committee of the Corporation determined that it was best, in view of the large number of students in the first year, that the instruction in military tactics should hereafter be given by an officer of the United States Army, as contemplated, although not required, by the act of 1862. They accordingly addressed the Honorable Secretary of War with the request for the detail of an officer for this service. In compliance with that application Lieut. Harry L. Hawthorne, of the Fourth Artillery, was assigned to service at the Institute, relieving in this duty Gen. Hobart Moore, who had for many years given the instruction in this department with the greatest care, pains, and effort, and with all the success it was reasonable to expect under civilian management.

Lieutenant Hawthorne entered the Naval Academy at Annapolis

in 1878, graduating in 1882. He was on sea service until June 1, 1884, when he passed his final examinations for appointment as an officer of the navy. The navy at that time affording opportunity for commissioning but a small part of the graduates of the academy, he was honorably discharged from the service under the act of Congress approved August 5, 1882. Passing examinations at once for the military service, he was commissioned second lieutenant of the Second Artillery in October, 1884. He took part in the operations against the Sioux Indians in the winter of 1890–91, and commanded the artillery in the fight at Wounded Knee Creek, December 9, 1890, where he was severely wounded. For gallantry and ability displayed on this occasion he was recommended for brevets of first lieutenant and captain, was mentioned in General Orders from the Headquarters of the Army, and was presented with a medal by Congress. He received his commission as first lieutenant August 28, 1891. Lieutenant Hawthorne entered upon his duties, with the rank of professor in the faculty, about the middle of March, and took up the problem of instruction in military tactics in a school of this character — for the problem must necessarily vary greatly with the varying character of schools — with much intelligence, spirit, and activity.

The substitution of an instructor in military tactics from the regular army, in place of an instructor from civil life, is of special importance at the present time by reason of the revolutionary changes recently introduced into the United States army tactics. So extensive are those changes, both in the form and in the spirit of the evolutions presented, that it must be a work of the greatest difficulty for any one not a professional soldier to completely master the new tactics. It is even a question whether it would not be more difficult for one who had been brought up under the old system, and become imbued with its spirit and familiar with its methods, to possess himself of the new tactics, than for one who had had no experience whatever in military matters. So diverse are the modern evolutions from those which they supersede, so hostile their spirit, that it is fairly a question whether any considerable part of the militia of the United States have yet come, or will for years come, to practice them according to their true military conception. Strongly contrasted as are formations under the two systems, that difference is yet not so great as is the difference in the relations between officers and men, and between the line and the staff. Lieutenant Hawthorne is by exceptional experiences peculiarly qualified to

take up this task of teaching the new tactics, in their methods and in their spirit.

The students of the Institute number 1,060 the current year, as against 1,011 in 1891–92. The last graduating class numbered 132, and with these went thirty or forty young men who had for four years enjoyed all the advantages of instruction at the Institute, but who had not attained its degree. A considerable number of students have also, as is usual, left the present second, third, and fourth year classes — some on account of deficiency of financial means; others from failure in scholarship; others from the exhaustion of the scholarly impulse; and others, still, with a view to accepting positions of a professional character. Except for the inevitable large annual loss from the first year class, due to the fact that many come to us each year who have no fitness for our work, or no disposition to serious study — the last indicated is the chief of the causes of the loss of students between one year and another at the Institute of Technology. Every year a considerable number of students leave at the end of their second or their third year to take professional positions. Some do this out of an impatience to get to work; others because they are unwilling to call upon their friends for further sacrifices on their account. Many of these, it is to be said, achieve no small degree of success, having learned while at the Institute to work hard when they work and to concentrate their powers upon the immediate object of study, and having acquired, also, no inconsiderable professional accomplishments.

The incoming first-year class numbers, of regular students 314, and of special students 48, making a total of 362. In addition to these, 80 have entered as regular or special students in the upper classes, making the total number of new men, 442. Our students come from thirty-nine States of the Union, besides the District of Columbia, Utah, and New Mexico; sixteen foreign countries are also represented on our lists, including England, Scotland, Ireland, France, Belgium, and Holland. Of the native students, 603 are from Massachusetts. Of these, Suffolk and Middlesex Counties send 185 each; Essex, 75; Norfolk, 72. One hundred and eleven Massachusetts towns send students to the Institute. The average age on entrance is eighteen years and ten months. Forty-nine graduates from institutions conferring degrees are included in our lists. Of these, six are our own graduates; twelve are from Harvard University; while twenty-five other colleges and universities are represented by one or more graduates. Forty-one women are students at the Institute.

The changes of the year affecting the several courses of study have, of course, been numerous, but few of them are of sufficient consequence to require description in this brief sketch. The most important of all is that by which the choice of courses has been made to take place at the opening of the second term instead of at the opening of the second year, as heretofore. This is a change which the faculty has long desired to bring about, but which has heretofore been withstood by one consideration or another. The advantages anticipated under the new rule are as follows:

(1) The students will come earlier under department supervision. Each student will, from the close of the first term, belong to a department, to the head of which he may resort for advice or needed assistance. The heads of departments, on the other hand, will begin looking after their own men one term earlier.

(2) The interest of the student in the school and in his own work will be increased as he feels himself a member of a distinct course, and sees himself ranked as such, instead of being merely one individual in a great unclassified body of students.

(3) The long novitiate of the first year will now be abridged one half, and the students will earlier get a taste of professional study, although the main part of their work will necessarily still be of a preparatory character.

(4) There will be a better adaptation of the studies of the second term to the subsequent work of each student. For example, heretofore it has been necessary to extend one and the same chemical course throughout the first year. Those who were in the result to go into an engineering course, or into architecture, have been obliged to take a somewhat longer and a somewhat different course than would have been given them in the interest of general training, or with reference to their subsequent studies. Men, on the other hand, who were in the result to enter some chemical course, such as chemistry, metallurgy, biology or geology, have been given a somewhat shorter and a somewhat different course than might have been thought desirable. In other words, the chemical work of the second half of the first year has thus far been in the nature of a compromise, precisely meeting the needs of neither the "chemical courses" on the one hand, nor the "mathematical courses" on the other. Under the new arrangement the chemical work of this term will be adapted to the requirements of each group of students. The architectural students will drop

chemistry entirely at the close of the first half-year, putting the time thus saved to use in drawing. The students of the course in "General Studies" will take physical laboratory work the second half-year in place of chemistry, securing thus, it is believed, a more varied training in experimentation and observation. The change fixing the choice of courses at the middle of the first year has also enabled the faculty to improve the instruction in first year drawing and descriptive geometry in several particulars.

The only other change in the courses of instruction to which I will refer is the very marked development of the option in naval architecture. This is a matter of such prime importance that I should feel called upon to speak of it at length were it not that the next number of the *Quarterly* will contain a full description of the course by Professor Peabody, to whom the development of this branch of instruction has mainly been due.

It remains only to say a word regarding the finances of the Institute. The Institute was poor last year, and the Institute is poor this year. Whether the Institute was poorer last year than this year, or is poorer this year than last year, its situation and needs being taken into account, are questions which it would require more mathematics than I have at command to answer satisfactorily. In any case the Institute is very, very poor. Magnificent possibilities of good work for the cause of education and for the development of the industries of Massachusetts, New England, and the United States, lie all around us. The only limit to our usefulness is found in our scanty means. For all which the munificence of the government or of private individuals shall place in its hands the Institute is prepared to return, "some thirty, some sixty, and some an hundred fold," to the State and to the community.

EUGÈNE LÉTANG.

By ARTHUR ROTCH.

Written for the Committee on the Department of Architecture.

The history of the Institute is almost an unbroken record of the appearance of men upon the scene at the most opportune moment. One of the most remarkable examples of this was the advent of Prof. Eugène Létang to its corps of professors at a time when there was in America no other opportunity for an academic education in architecture. Shortly before, in this, the country most needing trained architects, the Institute had opened its Department of Architecture and offered to those about to build up a new world an opportunity to know something beyond the rule-of-thumb office training of their predecessors.

Professor Ware, the first head of this department, after three experimental years turned to the École des Beaux Arts of Paris for help, and through his friends obtained the assistance of one of its most promising pupils. A strange and powerful personality was that of the foreign instructor, whose indifference to anything but his own personal work among his students estranged him at first from a general interest in courses of the Institute. He had no thought for ingratiating himself with his fellow professors, no time for theoretical plans for reorganizing his department or for extending it; for every day were not some of his pupils bent on drawing out some abominably irrational design? And was he not responsible for every bad example coming from the Institute? And so he fell with almost savage intensity upon the work of the students, and with few words of broken English, but endless graphic illustrations, forced upon each an inkling of the principles of rational constructive architecture. The task was almost superhuman. The traditions of the country at large and those of most of the profession were against him. A theoretical study of architecture was supposed by those who escaped from the drudgery of an office to mean a cultivation of the æsthetic imagination and a

discovery in the clouds of some original and beautiful American style. He sternly faced such enthusiasts with Socratic questions of why or what — "What is the purpose of your building, and how can you with your materials accomplish it and express it?" No eloquent aspiration, no longing for mere novelty, could turn aside his pitiless logic; and from the moment of his coming, when the Victorian Gothic was rampant, on through the years of Romanesque and Queen Anne fads, he brusquely forced his pupils along the lines of a logical structural style of design. Many were the revolts among the cleverer pupils, who were anxious to follow the style of the day; but few went forth from his teaching without a conscientious consideration of the beauty of truth and logic in design.

It is not too much to say that the result of Eugène Létang's intense scorn of sham and unconstructive designs, and of his admiration for a rational classic architecture, has been the means (through the graduates of the Institute) of changing the whole sentiment of the country, and has led to the erection in this style, with which he has identified himself and the Institute, of the magnificent group of classic buildings for the Columbian Exhibition at Chicago. These, designed by architects from all sections of the country, cannot fail to exert great and permanent influence for good on the future of architecture in America. But such fiery intensity working through personal and individual contact with the students could not fail to exhaust the nervous system of even Professor Létang's rugged constitution. His instruction was not through lectures, but was a personal wrestling, so to speak, with each student; and the result was an enormous outgo of nervous energy, followed by the inevitable reaction.

After the first half a dozen years he annually declared he could not serve another year, but as regularly returned to his post. He refused all professional opportunities of what promised wider fame and larger fortune. His whole life was bound up in his pupils; each was a drowning man whom he alone could save; and so year after year he worked on with diminished strength, but never lagging energy, till, after fourteen years, even his energy gave out, and then a vacation of one year was granted him. It was a crucial test. Could an influence so purely personal be removed without making a serious break in the department's work, and how far would the removal of his constant personal supervision lower the standard of that year's work? The question involved the ultimate value of his influence at the Institute. But his

old and devoted pupils rallied to the department, and at the end of the year the result showed no falling off from the preceding standard. This was a triumph most gratifying to all. It showed that his intense personality had formed traditions which were henceforth to become the heritage of the Institute, and which no longer partook of an uncertain personal character. It also proved that his vivid enthusiasm had brought about in a few years convictions and traditions which usually are the result of many years and many persons following a prescribed method.

After his vacation, Professor Létang returned to his work with new interest, and for the last five years the results have shown annually a higher standard and a wider scope. Throughout the length and breadth of the country his death was a personal shock to architects, and the most appreciative notices and tributes to the great work which he so modestly wrought have been published. With all the fine Gallic artistic sense which we might expect from a graduate of the École des Beaux Arts, he combined to an extraordinary degree a rugged sincerity whose effect upon others it would be impossible to overestimate. Esteem and a manly affection for him is the universal tribute which arises spontaneously from the hundreds of students who have passed forth from his instruction and are now the leading authorities in their profession.

The Institute most fully honors itself in honoring the memory of one who has contributed so much to the advancement of our national architecture.

Boston, December 14, 1892.

THE NEW TACTICS.

By Lieut. H. L. HAWTHORNE, Fourth U. S. Artillery.

Read November 10, 1892.

The changes that have been made in tactics have been made by leaps, either under the magic touch of a master soldier or by the more potent development of military necessity. The principal military influence has been the elaboration of the killing weapon; but owing to the sluggishness of the inventive faculty, or at least of that part which was devoted to the improvement of military arms subsequent to the times of Frederick the Great and the first Napoleon, the changes brought about by these men were allowed to remain undisturbed for many years.

The American Civil War marks the beginning of a very rapid growth in the improvement of tactics, brought about principally by the adoption and use of the breech-loading arm. Although this weapon was used to but a limited extent, still the lessons it taught were sufficiently distinct to cause the introduction of the single rank formation as being especially adapted to its use. The Austro-Prussian, the Franco-German, and the Russo-Turkish wars were replete with hints to the military leaders, and showed them plainly that the rapid loading arms not only seriously affected the old conditions, but gave rise to new ones on the field of battle, which necessitated changes in the methods of handling troops while subject to their fire. Since the Franco-German war all the leading nations of Europe have modified or changed their tactics once or oftener. The Germans have modified theirs five times, Italy and Russia each three times, and France and Austria each twice. The limits of this paper prevent me from describing more than a single phase of the subject of tactics. I shall select, therefore, that part relating to the maneuvering of troops on the field of battle in formations for attack, and attempt to show to what we have been led by the latest developments in this direction.

During our war, a brigade was composed of from three to eight regiments, and was usually formed for combat in two lines; the first line, covered by skirmishers, generally consisted of two or more regiments in line of battle; the second line was frequently similarly formed, but sometimes it was in close column of regiments or battalions, and at times in double column.

The arms then used could not prevent the skirmishers advancing within two hundred yards or less of a position with impunity, and there they awaited the coming of the assaulting lines, which kept an almost parade formation, sometimes not firing a shot until ready to attack with the bayonet. On the other hand, the defense waited the attack behind its breastworks, having the ground in front carefully prepared to delay or confuse the assaulting lines, where they would be held under deliberate fire as long as possible. In densely wooded districts, both sides would have their skirmishers in front, the defense giving ground until the opposing lines came so close — at times within twenty or thirty yards of each other — that the fire was as murderous and demoralizing as in any of the engagements in later wars, where both combatants used breech-loading arms. When muzzle-loading arms were used, there were always intervals with the companies when the firing almost ceased, as when the majority of the men were loading. During these intervals the captains had no difficulty in making themselves heard; a single company in close order covered a very small front, the men being shoulder to shoulder, and its commander could maneuver it with little difficulty; at critical moments, or where necessary, he could withhold the fire entirely or until it would be most effective; reinforcements took their places as units on the right or left of his command, and if necessary he could close his men to the right or left to make room for them; his control, comparatively, was complete. Now, however, with breech-loading arms it is the reverse; the fire once started is unceasing; the captain can rarely make his commands heard; he finds it difficult, if not impossible, to control the firing; his company is spread over a much more extensive front; he has to advance it over very much longer distances under a more severe fire, alternately deploying to decrease its losses when without cover, or rallying behind shelter to fire or resist the enemy's attacks; his men, scattered in hollows or ditches for protection, are loth to renew the advance, and finally when reinforcements come to his assistance he cannot close his men into the right or left to make room for

them; he finds himself surrounded by strange faces, his company disintegrated, and himself powerless. If this condition of affairs exists in the immediate presence of the captains, how much more powerless are those whose greater rank compels them to be farther to the rear and who are therefore less able to influence the results!

This utter want of cohesion in bodies of troops when brought under the fire of the modern rifle in the old formations was noticed early in the Franco-German war, and steps were taken by the German leaders, even in the progress of the campaigns, to make changes by which their losses were diminished. Since that time, and from the study of the Russo-Turkish war, military thinkers have about solved the best tactical methods of overcoming the difficulties enumerated above. These difficulties are illustrated in the accounts of the battles of the Franco-German and Russo-Turkish wars, where the marked features due to the use of rapid firing arms were: the disadvantages under which the superior officers labored in retaining control and direction of their troops while advancing to the attack; and the entire loss of control, sometimes even by the company officers, after the men were once engaged. This was due to the large size of the tactical units employed, their compelled deployment by the severity of the opponent's fire at distances varying from 2,000 to 3,000 yards from the enemy, and the dispersion of their component parts during the subsequent advance over these long distances. In the attack on the Rotherburg, for instance, "there were thirty-two companies of two different armies mixed up in utter confusion without unity of command." The difficulty of command is increased by the mixture of the different organizations, as we read that at Sedan "other Infantry detachments of the Eleventh Army Corps and of the Forty-sixth Regiment and Fifth Rifle Battalion had become mixed up with the Forty-third Brigade, and in the course of the engagement the regiments of the latter were likewise so intermingled that the officer in command of the brigade had at his disposal a mere multitude of men about one battalion (1,000) strong, composed of the most varied troops." The problem, then, for the attack of infantry would seem to be, how best to form and direct troops that they may be brought up to a defensive position without losing proper order and control, and to obtain adequate results for the men and material employed and at a minimum loss. The second consideration should be, how best to govern the fire

of the attack; but as this question would lead into the still unsolved problem of "fire tactics," it must be set aside for the present.

As we have seen, the control, under fire, by a single voice of so large a body of men as a company was found impracticable, and from this consideration there naturally followed the subdivision of the company; first into platoons, then sections, and finally squads, each under its own chief, yet held together by a common authority whose essential duty it was to direct and command. This alone would not insure proper control of the company, but by decreasing the amount of its front when deployed, by increasing its depth, the company commander could, from a central position, give direction to the attack, keep in touch with the *morale* of his fighting line, and give strength and confidence to his men by timely reinforcements. The attack in extended order does not, however, mean that a body shall suffer entire disintegration, but that its close order formations and rigid discipline should be maintained after the smaller groups have been formed.

The tendency towards dispersion could easily degenerate into a laxity fatal to the influence of superior officers, but it is at this point that careful military training asserts its power, and men are held and moved through an automism produced by a thorough peace preparation. Discipline must not be lessened by too much individualism. This seems to be the tendency of the German tactics. I am of the opinion, however, that the tactics we have now adopted stop short of total surrender to subordinates. It is believed that the more a man is taught to think and act for himself the less he is apt to be governed by the thoughts and actions of others. Our book, therefore, lays much stress on the careful training of masses. It furnishes us with a full list of maneuvers capable of filling all the requirements of actual campaigning, and enables the peace practice to develop a precision and order which any amount of extension of subdivisions cannot influence harmfully.

To show what changes have been introduced by the new regulations a brief comparison of the methods laid down by General Upton with those of the present book is necessary. Under the old system the battalion was the tactical unit, but could be divided into from two to ten companies. In preparing for attack a reserve was formed of two or three companies, and the size of company reserves designated by the colonel. The deployment as skirmishers was then made on a four of a particular company. In this way several hundred men

were spread out in a thin and more or less uncontrolled line over a large front, but held in hand directly by the bugle or the commands of one man. When this cloud of individuals entered the enemy's fire, all attempts at control beyond the signal for withdrawal ceased. The men acted entirely on their own responsibility, keeping up their fire, and, in spite of the slow loading process, usually expended their ammunition before the critical moment of the attack had arrived. No rules were laid down for the gradual strengthening of this line by judicious use of supports or of increasing the intensity of the fire. When it became necessary to reinforce, the entire reserve, including company reserves, were brought up at the same time, and being deployed as skirmishers a general mingling of companies took place. This, however, was not so vital considering that the colonel controlled, or tried to control, the detailed action of the entire battalion.

A later and, if possible, a worse method of deployment was introduced, by which the battalion was deployed by the numbers. There was thus created one, two, three, or four lines of skirmishers with half or more of the battalion as a reserve. The natural consequence of this method would be that these successive thin lines in the long advance under fire would lose their cohesion, either because of the difficulties presented by the ground or because the men composing them would naturally avoid the more exposed places and collect together behind cover. In availing themselves of this they would become crowded, and a large proportion of their fire be masked; the officers, owing to the mixture of strange men with their commands, would lose control, and advantage would be taken of this by the stragglers, when the line again moved forward, to remain behind and go to the rear. The second line would encounter similar troubles; and owing to the strong desire of the men to fire would soon be merged with the first, so that its value as a support would be of no practical benefit. This method of deployment had an advantage over the other, as the front covered was within the control of one man; whereas in the first case the front covered was so great that the line could not possibly be controlled when subjected to the fire of modern arms in the long advance.

It is perhaps fair to say that the authors of the old drill methods could not be expected to foresee the wonderful changes that have taken place since the introduction of the present infantry arm. In the days of Upton skirmishers were used to "annoy the enemy and exhaust his fire before the main attack, or to pursue him promptly

after a repulse." This last employment condemns itself when we consider that after a long and hotly contested advance the exhausted and excited skirmishers could scarcely be relied upon to conduct an orderly and rapid pursuit. In this sense, then, the advance of skirmishers was not in truth an attack. This was made, as before remarked, in what was commonly termed lines of battle; or, in other words, one or more solid lines of double rank moving at the charge.

It is plain that the conditions of the modern battlefield will permit of no such extension of lines, nor of the mixing of commands, nor of uncontrolled fire, nor of tardy and uncertain support. The new tactics must enable us to have closed formations in such small units that they can move with the greatest rapidity, give the maximum fire, and yet be under complete control; therefore there must be a greater subdivision of our tactical units to get the advantages of this dispersed order, and while their commanders are given more independence we must be careful, above all things, to leave the direction of the combat where it rightfully belongs — in the hands of the superior officers.

How does the new drill seek to accomplish this? The Board has wisely retained the regimental administrative unit. This is divided into three battalions of four companies each. The battalions have their own commanders, who are responsible for their instruction. No handier unit than the four-company battalion can be obtained for use on any battlefield. It gives a compact body of effectives numbering from 350 to 400 men more than the German or French company column, but more manageable and inherently possessed of greater power of discipline because of its permanent division into four companies, each with its commander. The captains must render direct and positive assistance to the field officers, and to make this more effective they have more independence than heretofore in carrying out their instructions. The captain in turn must have the assistance of his subordinates, who, to be responsible, must have direct command; therefore the subdivision into platoons is made and becomes a unit of importance for control and movement in the advance to the attack or on the fighting line. But even this body is too large to be controlled without assistance where it is so necessary to prevent a waste of ammunition; to have movements executed with the greatest rapidity; to utilize shelter and to prevent straggling. Therefore the platoon is subdivided — first into sections, and then into squads. But a front of four men was already the base of our system, so this unit

was utilized by giving it a commander — a non-commissioned officer — thus obtaining not only the equivalent of the foreign "fire unit" or "group," but also the active assistance, on the march and in battle, of our non-commissioned officers, by giving them direct control and responsibility. We thus do away with the system of numbering, which consumed time, determined the unit for the time being only, and necessitated reforming whenever the unit was broken up or became disorganized from any cause.

We have seen the incapabilities of the old organization when confronting the magazine long-range rifle. Now let us examine the methods which our present regulations have made possible, and enquire whether they will answer the requirements of the modern field of battle already specified.

The book begins at the very foundation training found and acknowledged by all the leading armies of Europe to be essential to success under the ordeal of the modern rifle. It says: "The squad is the basis of the extended order." This is now such a well-recognized unit that it needs no defense nor explanation. With this, then, as a basis, it naturally follows that its integrity should be preserved, and men be trained to regard it as the unit from which they ought never to be separated. Every effort is made to bring into prominence the importance of control down to the smallest group, and therefore the non-commissioned officer is raised into command and responsibility as a leader. Squad leading, then, is to be another important direction of development.

The object of this, we read, is "to prepare the squad for the battle exercises, by training the men to coöperate with their leader and conform instantly to his wishes." What a contrast to the old methods of the employment of our junior officers! If not in ranks, where their influence was no greater than any other file, they acted as file-closers, where they had "general charge of the men in their front to rectify mistakes and insure steadiness and promptness in the ranks, prevent men from falling out, etc."

But these directions, excellent in themselves, fixed such an indefinite responsibility that it was hardly practicable to enforce them. The non-commissioned officer, having no direct command, felt no responsibility; he failed to prevent men from falling out, either on the march or in battle, and in the latter case especially the evil increased to enormous proportions. In fact, at such times, the men in line, being

unaccustomed to having the non-commissioned officers in their rear exercise any control over them, paid but little attention to their directions, and not infrequently they were inclined to resent what they regarded as uncalled for interference. Now, on the other hand, just as soon as the sergeant or corporal is placed in the position of a section or squad commander, and is given direct control of a number of men, he finds himself with a distinct charge which excites his feelings of responsibility; he has no hesitation in trying to control his men, and they soon acquire a feeling of subordination and obedience to his commands; the captain and lieutenants thus receive the direct and positive assistance of the best men in the company in assuring the execution of their commands, and a firm basis is established for the preservation of their control in times of the greatest excitement and danger. I have elaborated this idea because I feel that upon this principle the whole fabric of modern tactics is built. Not only is it confirmed by the practice of foreign nations, who lead in military prowess and development, but the stories of modern battles furnish us with ample testimony in its favor.

The new drill lays down a carefully devised progression of training, beginning, as has been shown, with the men in squads and sections under non-commissioned officers; then as squads and sections under platoon leaders, with definite and distinct duties on the field of battle; then the gradual expansion of command, duties, and responsibilities up through the captains, battalion commanders, colonels, brigade commanders to the commander of the division. There is not a break in this succession, and the distinct powers of each are carefully laid down. It is even prescribed that under certain conditions of ground and cover the judgment of officers may be used, but this in turn is limited to certain definite movements or authority.

In contrast to what has been shown as the method of advance over ground under fire by the old tactics, let us picture the movements of an attacking battalion, one of a regiment organized under the present rules and governed by the principles laid down in the Infantry Drill Regulations of the United States Army.

A division of infantry about 10,000 strong awaits the completion of the preparatory stage of the combat to advance against a position on the edge of a wood some 3,500 yards in front. The ground is open, but affords shelter here and there in shallow ravines and gentle undulations. They are such, however, that a column could advance

along them but for short distances, thus compelling early separation into small bodies.

The design of the division commander is to make a powerful attack; he therefore orders the three brigades into the fighting line, instructing his brigade commanders to form their regiments side by side, each regiment in three lines, and gives them their order in line. The brigade commanders then issue the necessary orders for the above formation, designating the formation for the advance, leaving to the colonels of regiments to decide upon the moment for passing from close to extended order. The colonels then designate the battalions to form the fighting line, the support, and reserve; point out the position for the reserve; specify the formation for the fighting line and support; indicate the point at which the first deployment shall take place; and point out the objective and general direction of the advance. When the majors of the battalions chosen for the fighting line receive orders to attack, each designates two companies to form the fighting line and those for the support and reserve, gives instructions for forming line of sections or squads or deploying as skirmishers, and indicates the point of direction and the object of the movement.

The moment for the advance having come, the signal for the forward movement is made, and the entire division breaks cover and advances in the direction of the enemy in line of brigades, each brigade in line of regiments in column of battalions. The actual front of this mass of men is about 1,775 yards. The front occupied by the men themselves is about 1,450 yards. After a short forward movement the battalions of the support and reserve halt, the fighting battalions break into lines of companies in columns of fours and continue the advance. Any cover sufficient to accommodate any of these is taken advantage of, care being taken that the advance is not delayed. The battalions having reached the point indicated for deployment, each major gives the commands for taking the battle formation. The captains of companies for the fighting line designate the sections for supports and cause their companies to advance in the given direction, and at the signal from the major to take the battle formation. To do this each captain in the fighting line orders a few scouts sent forward, designates the non-commissioned officer to command them, indicating the objective to him and to the chiefs of platoons and sections. The captain then designates two sections, preferably the second and third, for the firing line, and when the scouts having gained about 150 yards

to the front gives the necessary commands for the extension. The supports halt while the firing line moves forward, and when at the designated distance from the enemy the lines are deployed into sections and then squads. The orders relative to the opening of fire will more or less modify the above conditions, but only as to the time of deployment and the rapidity of the advance. As I have pointed out, this question of fire tactics is so closely allied to the power of the weapon used that no settled rules concerning the fire have as yet been decided upon. Long distance firing is supported by some writers, and by others condemned. All unite on the employment of volley firing up to the latest possible moment, and that feature is a prominent one in our new regulations. The settlement of the question of the proper employment of fire will also decide the various distances to be taken in the advance by the supporting bodies and the reserves, and the points at which it will be necessary to take the various deployed formations. The drill regulations do specify the distances for the latter cases; but already the book has fallen behind the conditions created by the newest small arms. It seems to me that it would have been wise to have stated in general terms the advantages of certain fire and formations, specifying the value of the influences of ground, different kinds of cover, and character of the attack, rather than to have laid down hard and fast rules which depend on the power of so changeable a thing as a firearm.

When the squads can no longer advance as solid bodies, either by reason of lack of cover or because it is desirable to develop the fire of the attack more quickly, they are deployed as skirmishers, care being taken to avoid the mixing of sections and squads. The line of skirmishers soon overtakes the scouts, who join it. As the fire becomes more severe, advance from cover to cover by rushes is made by the whole line if possible. When the rushes begin it is an indication that the power of the attack is waning, and it is at this stage, therefore, that the moral effect of the supports is made to tell. They join the firing line successively in groups, giving it momentum for the next rush and renewing its courage. The handling of the supports and reserve is where the highest type of fighting ability will show itself. The brigade commander has full control of the regimental reserves, leaving to the colonels the employment of the supporting battalion. The reserve of the battalion receives its orders from its

commander only, who may also hasten or delay the reinforcement by the company supports. This may be left to the judgment of the company commanders, but it is a wise provision that places this also in the hands of the commander whose greater distance from the disturbing influence of the fire and whose wider field of view leave his judgment cooler and broader.

As the opposing fire grows in intensity the unity of the attacking line breaks more and more, throwing on the small group commanders more and more responsibility for the continuance of the advance. The rushes are by small groups, pushed now and then by reinforcing bodies, and covered in the short advances by volleys delivered on either side. The company supports have been absorbed and part of the battalion reserve joins the firing line, while the battalion in the second line advances nearer under cover of the rapid fire which is now opened. If the rapid fire does not shake the enemy, the remainder of the battalion reserve is quickly brought up, and another rush made, followed by a rapid fire. During this fire the battalion of the second line reinforces the firing line; at the signal from the colonel the field music sounds the charge, and the whole line rushes upon the enemy. The third battalion still remains, on which the attacking troops may rally if repulsed, or by which a pursuit may be attempted or a counter attack resisted.

Assuming forty paces for the front of a company, then observing the proper intervals between subdivisions, the front of a regiment in columns of battalions would be 169 paces. The attack of this regiment must cover this front and half the interval between it and the regiments on its flanks, or 48 paces. Three battalions of 380 men each are therefore made to attack a front of 217 paces.

The depth of the attacking column is such that by decreasing in this direction it will make up not only for all losses in the advance, but will gradually increase the intensity of the attacking fire and make the assaulting line so solid that it is no longer a line or swarm of skirmishers, but has all the momentum and confidence of the old shoulder to shoulder line of battle. We thus see that assuming a loss of 40 per cent. for the fighting line and a loss of 20 per cent. for the supporting battalion during the advance, we shall still have for the final charge 912 men to 217 paces or more than a solid line of four ranks.

The new drill teaches much more than this single phase of the business of the soldier, but the excellence of this ought to give us confidence as to the worth of the rest. It is hard to draw the line between drill and tactics. To go into the minutiæ of minor tactics would have required a volume many times larger than the present one. The literature on this subject is already confusing in its richness, and the military student finds the different phases and branches of the military art growing sufficiently broad to make each a distinct specialty.

A final word as to the question of the importance of drill. It is becoming too fashionable to regard drill as the meager mental limits of the half informed subaltern. Those who entertain such an opinion are usually men who have never experienced the actual difficulties that arise in taking the very first step, with poorly drilled men, in the execution of the simplest movement in the more advanced science called minor tactics. There is a fascination about the working out on a map, with a pointer, the puppet movements of assumed hostile bodies. The human beings who compose these bodies are not thought of. They do just as the drill books says they should do, so all bother from that direction is taken from the mind of the theorist and he goes merrily on reading, first how to fight a company, then a battalion, then a regiment, then a body composed of all arms. In the meantime his opinion of the art of making the solution of these problems possible grows more and more uncomplimentary, and the real education of the soldier is left to those who are regarded as unfit for anything beyond the duties of a taskmaster.

All masters of their art acknowledge that the more the extended order becomes our battle formation, the more important will practice in close order movements become as a means of training for the individual soldier. Just as the one order becomes more necessary to us, so the other at the same time gains consequence.

The value of the solidity of the battalion column, of readiness in passing from one formation to another, of individual dexterity, increases in like proportion to the necessity, under difficulties, for change from the one to the other form of battle. The so-called "close order drill," that is to say, the habit of more complete order and subordination at any given moment, will therefore gain importance from the new requirements of tactics.

Von Scherff says: "The power of moving surely and with cohesion in close order stands first and foremost as point of departure, and also as keystone of all action in battle. The ability to pass quickly from one form of close order to another and again to change from close to extended order and *vice versa* come next. Mobility, attention to orders, fire discipline, constitute the third requirement. . . . Lastly, a general acquaintance with the forms and requirements of outpost duty. These," he says, "will make infantry equal to every emergency of the greater operations of war." If all this be regarded as an inferior occupation for an officer, and avoided as too insignificant for the students of the higher branches of the art, then when the trial of strength comes those soldiers will be found wanting whose training and drill have been conducted unintelligently or neglected altogether.

What we must demand of the soldier is discipline, born of high personal energy. We must require of him a perfect familiarity with the use of arms. We should seek to develop his skill in taking advantage of the ground. Theorists cannot or will not do these things, but the practical soldier knows the full meaning of it all, and will make this work his first care.

Foreign drill books give brief directions how to work on a man's imagination. They are confined to a few simple principles for fighting, and are calculated to sharpen his wits and raise his self-respect. To the credit of our Board be it said no such gratuitous suggestions were made in the Regulations. To any one who has served in any army of the United States, regular or volunteer, a cure for excessive imagination and exuberant wit in our men would have been looked upon as the more necessary. No one has yet been able to give a good recipe for the production of *morale*, so our new book does not attempt it.

Boguslawski says: "A few great principles must be plain to all. These are that rules should be few and brief, as an attack must be made unswervingly and without hesitation; patriotism must be awakened by means of instruction; officers must give an irreproachable example, both in conduct and bearing, and they must raise the soldier's self-respect by treating him as an honorable man. If these ideas be carried out so far as is compatible with rigid discipline, much will have been done to develop military virtues in the soldier and to prepare him for the great tragedy of battle. The soldier must be mentally

and physically trained for war. Ingrain such training, and make it his second nature, even in the heat of battle, to keep touch with his officers and comrades. At the same time cultivate the man's mental powers and teach him to think for himself. War and the whistle of projectiles have a serious effect on the nerves of most men. So great is the instinct of self-preservation that some men completely lose their heads. The soldier must pursue his calling in the face of death. Such is his *raison d'être*, often forgotten during a long period of peace."

WEIGHTS AND MEASURES OF THE UNITED STATES.

By Prof. T. C. MENDENHALL, U. S. Coast and Geodetic Survey.

Abstract of a paper read November 22, 1892.

The Customary Weights and Measures of the United States, that is to say, those commonly in use in this country, are either identical with or are derived from those which were lawful during the colonial period. Of necessity, then, they are almost entirely of English origin.

The Constitution gives Congress power to establish a system of weights and measures and to fix standards of the same. In the early days of the Republic much interest was shown in this subject by the most eminent men of the time; prominent among these may be mentioned George Washington and Thomas Jefferson, each of whom earnestly recommended action by Congress in this direction, the latter having devised certain very notable improvements in the system of weights and measures then in common use. The subject received the attention of Congress and the Executive Departments from time to time, but up to about the year 1828 there was no legislation upon the subject. In the meantime it became necessary for the Executive Departments of the government to establish some system of standards of length, mass, and capacity, in order that they might be governed thereby in the collections of customs and taxes.

The Treasury Department being most largely concerned with these matters, the question of providing and fixing standards was early confided to Mr. Hassler, the first Superintendent of the Coast and Geodetic Survey, and from that time to the present the Superintendent of the Coast and Geodetic Survey has been the Superintendent of Weights and Measures.

For the use of this Survey in its early operations Mr. Hassler had brought from Europe early in the century a brass bar, a part of which became and was for many years the unit of length adopted by the Treasury and other Executive Departments of the government. Congress not having acted upon the subject of weights and measures, the matter was left in the various States to local and legislative control,

and consequently the practice was exceedingly irregular. In many States, indeed, units of length and mass and other standards were merely traditional, and had no force based upon legislative action or other higher authority. While such confusion as existed might be tolerated in ordinary private commercial transactions, Congress saw that it was wise to establish at least a standard of weight which might be used in determining the mass of coins. This led to the passage of the act of May 19, 1828, which is the first, and indeed almost the only definite act of Congress upon the subject of weights and measures. This act provides that a certain piece of brass, now known as the Mint Troy Pound, should constitute the standard by means of which the coinage of the United States should be regulated. This standard, judged according to our modern ideas, is exceedingly imperfect, both as to its form and as to its construction. It is not solid, and consequently it is impossible to determine its density with any degree of accuracy, and hence the effect of variations of barometric pressure upon its weight must remain unknown. It was a copy, and doubtless a very precise copy, of the British standard troy pound then in use. This British standard, however, was a few years later destroyed in the burning of the Parliament House, and a new standard pound was constructed to which reference will be made later.

Although Congress was apparently unwilling to legislate in order to secure and require uniformity of weights and measures throughout the country, it seemed to be not unwilling to encourage this uniformity, and the next act, which is that of June, 1836, is of that character. It provided that a full set or a copy of all the standards of weight and measure which had been provided by the Treasury Department for the use of the Customs Service should be prepared at the Office of Weights and Measures and delivered to the Governor of each State in the Union. This was accomplished by the Superintendent of Weights and Measures during the next few years, and the action went far to secure uniformity in the customary weights and measures by the fact of the adoption of these material standards as received by the respective States for the standards by the Legislatures thereof. Some of the States had previous to this time adopted standards which differed slightly from these of the Treasury Department; but in all cases after a few years such legislation was repealed, and the standards adopted by the Executive Departments of the government soon became practically the standards for the whole country. Of these the primary units were the yard and the pound.

As a good deal of confusion has existed from time to time in regard to the relation of these units to the corresponding units of the British government it may be well to refer briefly to this relation. It has already been stated that the yard adopted was the distance between two lines drawn upon the scale brought over by Hassler about the year 1813, and known as the Troughton scale. It is true that the length of this yard from time to time has had different relations to that of the British yard, owing entirely to the temperature at which the comparison has been assumed to be made. It has probably never been the policy of the Office of Weights and Measures, however, to have a real outstanding difference between this yard and the English standard. As there is not and has never been legislation regarding the temperature at which this standard is to be accepted, it is clear that it is wise to fix the temperature at such a point that it may be equal as nearly as possible to the British standard yard, provided, of course, this may be done within a reasonable range of temperature at which comparisons may ordinarily be made. At 62° Fahrenheit, at which the yard was for a long time assumed to be a standard, it was found upon comparison to be longer than the English standard, that is to say, the present Imperial standard yard, and it was therefore determined to alter the temperature at which it was considered to be correct by reducing it to 59.6 instead of 62, at which point it is, as far as can be known, in accurate agreement with the present Imperial standard of Great Britain. It is hardly necessary to say that these two standards have never been compared directly with each other, but by means of copies of the Imperial standard which have been furnished the United States by the British Office of Weights and Measures.

The standard of mass adopted by the Treasury Department, and in virtue of the copies distributed under act of Congress becoming the standard for the States of the Union, is the avoirdupois pound which was derived from the mint pound already referred to. As stated before, this mint pound was a copy of the British troy pound of 1758, and as the new Imperial avoirdupois pound of Great Britain is derived from the same standard it may safely be said that there is little outstanding difference between the avoirdupois pound of Great Britain and that of the United States. In fact, certain comparisons by means of intermediate standards show this difference to be so small that it is the practice of the Office of Weights and Measures to assume the two pounds to be identical. Thus it may be properly declared that the

yard and pound of the United States are practically identical with the yard and pound of Great Britain.

Capacity measures were also adopted at this time by the Treasury Department, consisting of the wine gallon and what is known as the Winchester bushel. These are theoretically derived from the standard of length, the former containing 231 cubic inches and the latter, the bushel, containing 2,150.42 cubic inches. These are old colonial standards, and differ considerably from the present standards of capacity of similar names in use in Great Britain.

The origin and growth of the metric system during all of these years is familiar to everybody. The superior character of this system both as to simplicity and scientific precision was recognized at an early day by those who gave attention to the subject of weights and measures in this country, but as it did not seem for a considerable time at least that this system, notwithstanding its great excellence, was likely to receive general international approval, or even locally the approval of the French nation, no very serious movements took place towards its introduction in this country. A little after the middle of the present century, however, it became evident to all intelligent observers that this system was destined to become in time, and at no very distant time, the great international system of weight and measure. It had already come into extensive use among scientific men of all nations and even among English-speaking people, and serious efforts began to be put forth to secure its recognition by legislation in Congress. The result of this agitation of the question was the passage of the very important act of July 28, 1866, in which the use of the metric system of weights and measures is authorized and made lawful throughout the United States of America. In the same act a set of tables was adopted as establishing in terms of the weights and measures now in use in the United States the equivalents of the weights and measures expressed therein in terms of the metric system.

Although incomplete and unsatisfactory in itself, this legislation is, and in great degree for that very reason, most important and far-reaching in its results. At about the same time, in order that the practical use of this system might be encouraged and aided, Congress passed a joint resolution authorizing the Secretary of the Treasury to distribute to each of the States of the Union a set of Metric Standards of Weight and Measure, and at a later date similar sets together with the standards of customary weight and measure were distributed

to all of the colleges of the country founded upon the National Land Grant.

In the meantime it was discovered that the copies of the metric standards which were in use in various parts of the world by different nations were not in perfect agreement with each other, and that their relation to the original prototype metre and kilogramme was not known in a manner satisfactory to the requirements of modern science. Indeed, the original prototypes themselves, the metre and kilogramme of the Archives, were not constructed with that perfection and precision which was possible by modern methods, and did not themselves meet the requirements of modern metrology. The use of the metric system was spreading rapidly throughout the world, and it became important that the entire system of standards should be revised in a proper manner, and to this end an international commission was organized at the invitation of the French government. The result of this was the final establishment of a permanent International Bureau of Weights and Measures near Paris, which is supported by contributions from nearly all the civilized governments of the world and controlled by representatives of these same governments.

After nearly fifteen years of labor, including much original investigation and research relating to the principles which should control standards of weight and measure and many experiments as to the best methods of constructing and intercomparing such standards, the International Bureau had, in 1890, successfully completed the preparation of a considerable number of copies of the original metre of the Archives and of the original prototype kilogramme. These had all been compared with the original standards and with each other with the very highest degree of precision attainable by the best modern methods, and their differences, while small, were known with a great degree of accuracy. That metre which agreed most nearly with the metre of the Archives, and that kilogramme which agreed most nearly with the kilogramme of the Archives, were selected as the great international prototype units of length and mass. The other copies were distributed by lot to the various contributing nations, and thus the United States came into possession of two copies of the metre bearing respectively the numbers 21 and 27, and two copies of the prototype kilogramme, bearing the numbers 4 and 20. The first received at the Office of Weights and Measures in Washington were the metre No. 27 and kilogramme No. 20, these having been carried from Paris with

great care by special messenger. The seals upon the cases containing them were broken and the standards examined on the 2d of January, 1890, by the President of the United States, Benjamin Harrison, in the presence of the Secretary of State, James G. Blaine, and the Secretary of the Treasury, William Windom, all of whom attested to the identity and condition of the standards referred to. In virtue of a certificate to this effect, signed by these three distinguished officials, these standards have become the national prototypes for the United States.

In reference to these prototypes, action has recently been taken by the Office of Weights and Measures, which, while practically it has little or no bearing upon the value of the customary weights and measures in this country, yet from a theoretical standpoint must be regarded as of great importance. Referring again to the act of July 28, 1866, it will be remembered that in virtue of this legislation the use of the metric system was made lawful throughout the United States of America. No such action has ever been taken by Congress in reference to the customary system of weights and measures, and the authority of the yard and pound to-day rests entirely upon local legislative action. It will thus be seen that by the act of 1866 the metric system in the United States is placed upon a basis entirely different from and indeed quite superior to that of the customary system of weights and measures. Indeed, it is the only system of weights and measures which has ever been made lawful by act of Congress, and in this the use of the mint pound according to the act of 1828 is not excepted, for that was made lawful only for purposes of coinage. It has already been stated that the act of 1866 contained certain tables giving the equivalents of the weights and measures as ordinarily used in terms of the metric system. Nowhere in this act is the metric system defined. It must therefore be assumed that the word "metric" is used in the sense in which it is universally used throughout the world when applied to a system of weights and measures, that is to say, that the system referred to is that in which the metre and kilogramme of the Archives, or now the international prototype metre and kilogramme, are the standards of length and mass.

As Congress has never in any manner defined a yard or a pound, or adopted any material representation of these units, the best that can be said for them is that the words are to be used in the ordinarily

accepted sense. We have on the one hand, therefore, a system of weight and measure which has received the approval of nearly the entire civilized world, the fundamental standards for which have been constructed in accordance with the highest requirements of metrological science and which has been authorized and made lawful for the whole United States by special act of Congress. Further than this, Congress has for many years authorized and sustained the participation on the part of the United States government in all the operations of the International Bureau of Weights and Measures, in which the metric system was being and is being perfected and perpetuated.

On the other hand we have a system of weights and measures whose only recommendation is that it has been for many years in customary or common use. It is irrational in theory, irksome in practice, and has been condemned by all who are competent to speak upon a subject of this kind. Furthermore, it has never been authorized or made lawful by act of Congress, and as a matter of fact is almost without recognition in the history of congressional legislation. It has been determined by the Superintendent of Weights and Measures that a proper and wise recognition of these facts justifies, if it does not demand, the acceptance of the international prototype metre as the unit of length, and the international prototype kilogramme as the unit of mass — their representative material standards now in the possession of the government being utilized for such comparisons as may be deemed necessary and expedient. This implies that the yard shall hereafter be defined as a certain fraction of a metre, and that a pound shall be defined as a certain fraction of a kilogramme, and this will be the practice of the Office of Weights and Measures henceforth.[1] While not interfering in the slightest degree with the practical or commercial use of the yard and the pound, there are very important theoretical and scientific advantages in thus referring these units to the great international prototypes, which are already the standards of length and mass for the greater portion of the civilized world. It is hoped, therefore, that this action will meet with the approval of all who are interested in the advancement of the science of metrology and in the increase of precision in both theoretical and applied science.

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[1] These definitions, according to the act of 1866, are as follows:

$$1 \text{ yard} = \frac{3600}{3937} \text{ metre.} \qquad 1 \text{ pound} = \frac{1}{2.2046} \text{ kilogramme.}$$

LIGHTNING AND STRONG CURRENT ARRESTERS AS USED FOR THE TELEPHONE.

By I. H. FARNHAM.

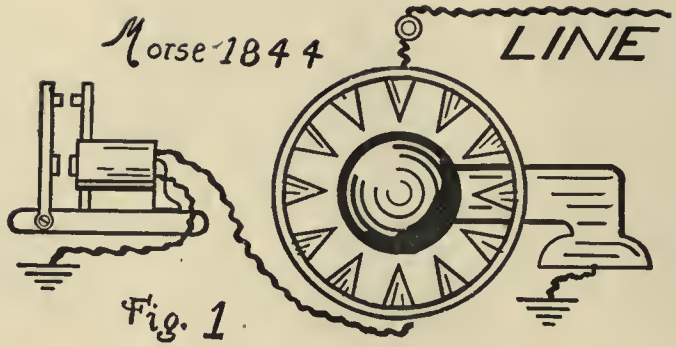
Read December 8, 1892.

When Franklin proposed to erect lightning rods which would convey the lightning to the ground, and so protect buildings to which they were attached, there were not wanting superstitious people who proffered all sorts of warnings. Some argued that it was "as impious to ward off heaven's lightnings as for a child to ward off the chastening rod of its father." Others went to the opposite extreme and concluded that if there was any impiety about lightning rods it lay in the fact that they invited the "chastening rod," and that it was madness to "tempt Providence" in any such way.

The foregoing is substantially one of the records given in connection with the first attempts to prevent the danger of the lightning's stroke as long ago as 1752 (Age of Electricity). From that time to this the sons of Franklin have been seeking the best means of diverting the discharge of nature's great "Leyden jar" away from people and property.

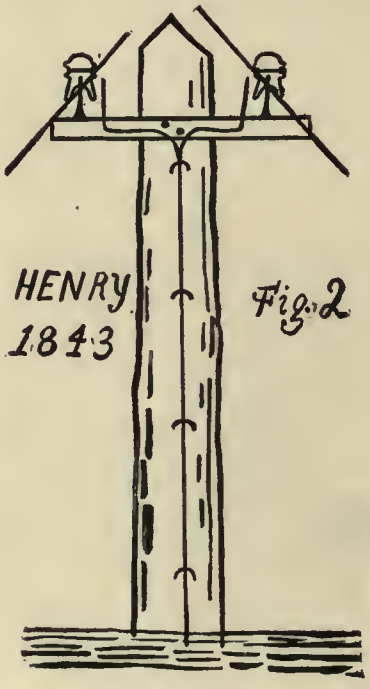
The necessity of protecting telegraphic instruments from lightning was forced upon the pioneers in this art, and considerable ingenuity was exercised in those early days upon the problem. It will be interesting before passing to the direct subject of the evening to glance at these earliest forms of protectors.

The first lightning disturbance on electrical instruments was noted on the telegraph line laid down at Göttingen in 1833 or 1834. It was ten years later, however, that Professor Morse, encountering the same troubles with his first line, was compelled to cast about for some remedy.



Among other devices of his was that shown in Figure 1. It consisted of a brass ball, surrounded by a metal ring, which ball was connected with the ground. The ring had on its inner surface a number of small points projecting towards the ball, but not in contact with it; it formed a part of the line circuit.

In 1846 the American Philosophical Society called the attention of Professor Henry to the subject. He made careful deductions from his study of the difficulty, and to him is credited the plan of running a wire from a ground plate up the telegraph pole and ending it about a half inch directly beneath the telegraph wire, as shown in Figure 2.



This method of protection is employed to-day on many lines, though it serves only to arrest the very heavy discharges, the space between the end of the ground wire and the line wire being too great to take away ordinary or, to the instruments, even dangerous discharges.

Steinheil, in 1846, used metal plates 6″ in diameter, separated by thin silk cloth; these plates were placed on the roof of the station and protected from rain by a wooden box. This arrester proved quite satisfactory.

In 1847 a French electrician, Brequet, proposed using a very fine wire from the station to the main line, 15 or 18 feet distant. This was expected to fuse on the passage of lightning, and thus arrest it before entering the station.

Those early workers in the electrical field had only nature's lightnings to guard against, while we meet to-day a much more difficult problem in guarding our delicate apparatus from many forms and degrees of strong currents down to the well-named "sneak" current, which steals in over all the early forms of protectors and silently consumes the apparatus.

It is the combination of difficulties found in guarding against this wide range of troublesome currents, without at the same time rendering the instruments insensible to their legitimate currents, as well as to outline the results so far accomplished, particularly as applied in the telephone field, that interests us to-night.

There are now about 350 different American patents on lightning and heavy-current protectors. We will mention only a few characteristic forms. They may be divided into several different classes, each class embodying individual and distinct features. There are forms which open the line circuit, and so shut out the dangerous current; and others which offer to the heavy current a shunt or bypath around the instrument to be protected. Again, they could be divided into single-acting and multiple-acting; that is, devices which after being operated are automatically readjusted for further service, and those which require manual resetting.

I prefer, however, to arrange them in four classes, as follows:

1. Air space, or spark arresters.
2. Fuses.
3. Magnetic Devices.
4. Thermostatic Devices.

Some protectors embody more than one principle in their operation, and so cannot be strictly confined in any one of these classes.

The "*air space*" *arresters*, the first class, include Professor Morse's ball and pointed ring and Professor Henry's wire ending at a point near the line on a pole; also the plate arrester as modified by the silk cloth of Steinheil, and the paper separators of other inventors — a large variety of forms well adapted to divert lightning from the lines to the earth. One form, a two-plate arrester in common use by telegraph companies, has brass line plates separated from a brass ground plate by a rubber ring about $\frac{1}{32}$ inch in thickness, allowing an air space between the line plate and the ground plate through which the high potential charge of atmospheric electricity passes as a spark. [In the model shown several spots could be seen where the brass had been fused by the discharge.] In fact, it is the liability of the metal plates fusing together and thus intercepting the passage of the working currents that has induced many persons to try other means of protection. The insertion of varnished or paraffined paper between the plates sometimes, but not always, prevents the fusing of the metal together.

In several other forms paper is used between plates; an arrester having a heavy copper rod set into a groove in a wooden base, with strips or plates of brass held against the rod, separated only by thin paper, has proved a very efficient lightning arrester. I have sat during a thunderstorm in a wire tower having 400 lines passing through a set of these arresters, and was interested to hear the little discharges take place, now on one line and again on others, sometimes accompanied by the lightning flashes which evidently caused them.

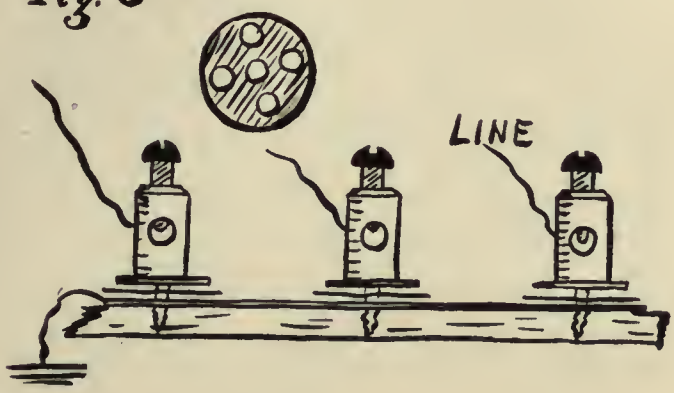
In other forms, again, these arresters have points on one or both plates. Arresters provided with such points are supposed to be superior to those having plain surfaces, but in practice it is difficult to discover the advantage.

It should be remembered that air space arresters in order to be effective must have a very small space through which the discharge is to take place. Failure to recognize this fact has caused the loss of many instruments, and was the reason that Professor Morse and Professor Henry's air space arresters were not as satisfactory as hoped. It is for this reason, too, that many of this class of arresters, having paper or cloth separating the disks or plates, have proved more satisfactory than the strictly air space form. In the latter, manufacturers and

users have failed to place the plates or points near enough together, fearing actual contact.

The high potential current will spark across the air space more readily than through paper or other material; hence, other things being the same, the purely air space is the better, though, as has been stated, until very recently the paper-separated plates have proved more reliable in practice, on account of the very small separation of the line and ground.

Fig. 3



The arrester shown in Figure 3 combines the advantages of both the purely air space and the forms having paper separators. It was designed by a Mr. Bowers, of Fitchburg, Massachusetts, and probably was the first to embody the use of perforated paper or other insulating material between the arrester plates. This arrester has done good service, as can be seen from an examination of a strip of plates which has been in practical use. It is interesting to note that the discharges have taken place in every instance through the openings in the paper and not through the solid body of it.

Let us now look at a modern air space arrester constructed on the principle of the one just examined, but possessing several advantages over it. The plates are of carbon, and therefore non-fusible. Having a roughened surface, they contain whatever advantages there are in point arresters. The plates are separated by a sheet of perforated mica. The mica is of such a thickness (about .0035 inch) as to allow a discharge to take place across the space when the difference of potential between the plates reaches or exceeds 300 volts. This

arrester has been constructed in several forms. It was found in service that occasionally particles of carbon dust were dislodged from the plates at the air space, thus connecting them together. The arresters are now so constructed that the plates may be readily withdrawn from the clips which hold them in position.

The "*fuse*," our second class, was suggested first by Brequet, in 1847, as a lightning arrester. A fuse has little value as a protector from lightning; the discharge being so sudden and disruptive that by the time the fuse has operated the enemy has struck the fatal blow and vanished.

Fig. 4.

Figure 4 shows an arrangement of fuse and lever designed to open the main line from the instrument and connect it to the ground in season to save the instrument and dispose of the lightning discharge. Quite a number of arresters have been devised on this principle. Such apparatus is "too slow" for lightning.

Fuses, however, have found their value in protecting against continued currents — such as are diverted from electric light and power wires.

Of fuses in common use, the "Vail," consisting of a very narrow bit of tin foil protected in a hard rubber tube, and mounted to be easily replaced by a fresh tube and foil, was one of the early forms employed by the telephone companies. It was intended to operate with about $\frac{1}{2}$ ampère of current. This protector was very useful in cutting off heavy currents, but was found wanting in two essentials. First, it would often allow a current just strong enough to destroy an annunciator on the switch board to pass it, while very small atmospheric discharges, too feeble to cause any trouble whatever to the apparatus, would open the fuses by the score.

The "Hibbard" protector — a foil wound up in asbestos paper and held between German silver clips, has also been much used, and has saved many offices from damage and possible fire, but is found to have the same objections as presented in the "Vail." German silver wire of very small diameter has been used to a limited extent, sometimes being stretched through glass tubes.

A form of protector embodying both the fuse and plate arrester has been used. It consists of a fine copper or German silver wire wound about a metal rod; the wire forms a portion of the line circuit, and is separated from the rod by paper or other thin insulating material, the rod, of course, being connected with the ground.

The third class comprises a large variety of "*magnetic devices.*" Some are designed to open the line, while others shunt the instrument to be protected through a heavier wire, usually that around the protector magnet itself. The use of the magnet has been an inviting field for protector inventors, as by its use, and scarcely by any other means, has it been deemed practicable to construct an automatically restoring protector for continuous heavy currents. Figure 5 fairly

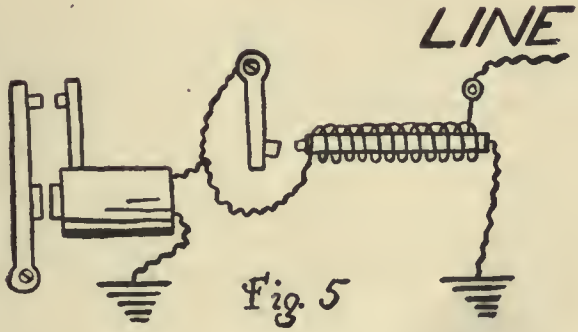


Fig. 5

illustrates one of these arresters designed to shunt the instrument through the comparatively coarse wire around the protector magnet. It will be understood from the illustration that the ordinary working current will not operate the armature of the protector. A heavy current, however, will bring the protector armature against the contact point placed on the magnet core and form a shunt around the telephone or other instrument to be guarded. This instrument is sometimes modified by being provided with two or more distinct coils of wire, each one differing in sensitiveness from the others, the object being

to cause the armature located in front of the coil, having many turns (necessarily small wire), to operate with moderately strong currents, while the armature of the coarser coil and less number of turns responds to currents that would endanger the smaller wire. Figure 6 is intended to show a form of this instrument. The "Cartwright" protector presented is constructed on this principle.

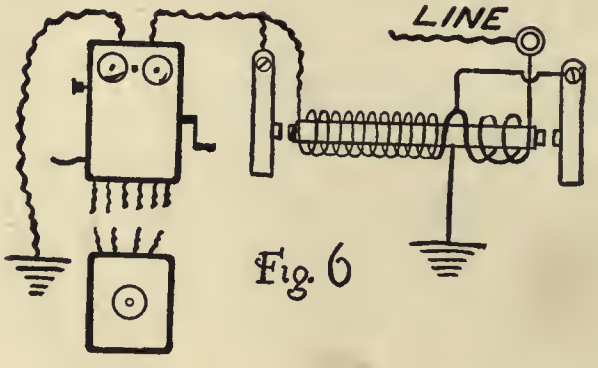


Fig. 6

When arc light circuits were the only ones liable to come in contact with wires conveying feeble currents the form of protector just described was quite satisfactory; in fact, it was extensively adopted for electric fire alarm systems. It is readily understood that this protector is self-restoring or multiple-acting, because the moment the abnormal current ceases the protector armature returns to its usual position. Arc light dynamos automatically adjust themselves to deliver a current limited to nine or ten ampères, and this would not endanger ordinary office and line wires, or even the wire used in the form of protectors just described; but this is not true of railway and other power dynamos, which may yield a volume of current sufficient, if allowed to continue, to destroy any wire employed either in the protectors themselves or the connecting lines. For this reason wherever this form of protector is used to-day it becomes necessary to add a fuse wire in the circuit to protect the protector itself against very heavy currents.

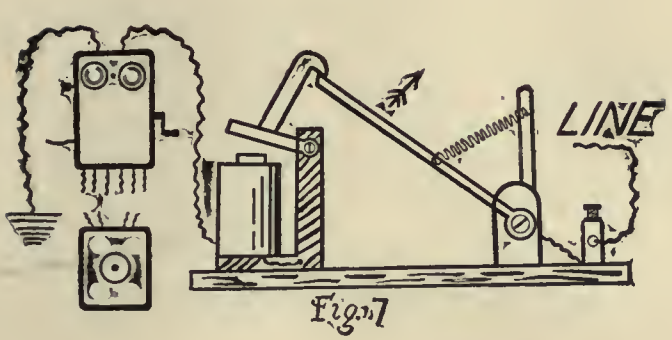


Fig. 7

A magnetic device for opening the circuit is shown in Figure 7. There are a number of similar forms. This is called the "Plush," from its inventor, Dr. Plush, of Philadelphia. It is extensively used by the telegraph, and to a limited extent by telephone companies. As a protector from dangerous currents which continue upon the line until they are interrupted it works admirably. Its weakness lies in that, when it is so adjusted as to operate with moderately dangerous continuous currents, it is operated by discharges of lightning so small as to cause no danger; and, as it is not multiple-acting but leaves the line open until manually reset, frequently occasions delay. Located on grounded lines in central offices, where attendants are always at hand, it is a good protector.

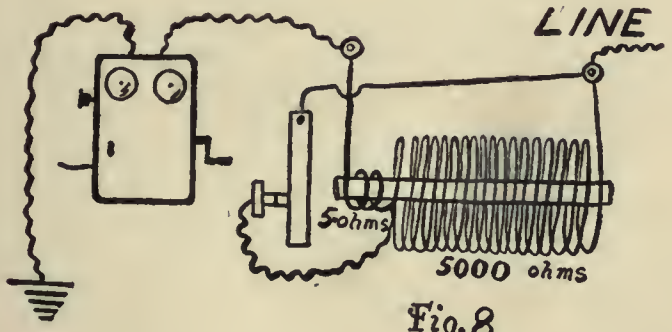


Fig. 8

Figure 8 illustrates the operating principle of an ingenious protector brought out by Mr. Clauson, of Boston. It might be termed a compromise between the last two forms described. To a great extent

it has the advantages of both, without the faults of either. The magnet is surrounded in this case by an ordinary coil of copper wire supplemented by another coil of very high resistance. The armature, when standing in its normal position, shunts the high resistance coil from the line circuit. When, however, a heavy current passes through the low resistance coil the armature is moved forward, thus bringing instantly into the circuit the high resistance coil, which by its resistance reduces the dangerous current to an amount safely carried even by its own fine wire. There is at the same time sufficient foreign current allowed to flow to retain the armature in the "safety" or protecting position. When the source of the dangerous current is removed the armature automatically returns to its back stop again, shunting the now unnecessary resistance from the line. Mr. Clauson has constructed several modifications of this protector, in one of which he employs carbon resistance.

It would seem at first thought difficult to construct a magnetic device which would not be responsive to lightning discharges and yet would operate with the much feebler currents, even "sneak" currents. This has been accomplished, and is illustrated by Figure 9.

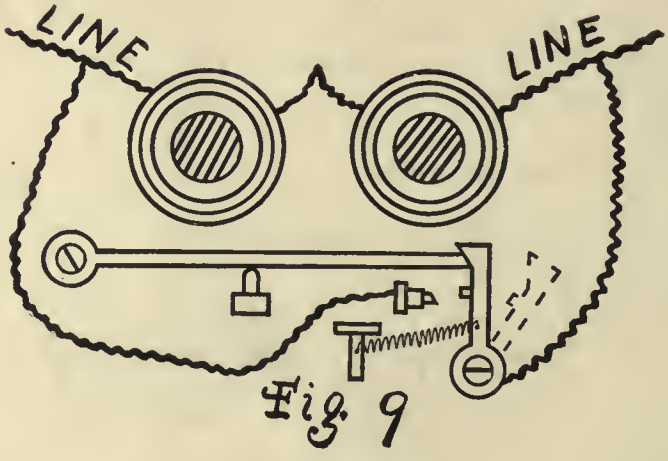


Fig. 9

The electro-magnet shown in this figure is supposed to be the regular working magnet of the instrument to be protected — a telephone bell, for instance. Beneath, or at one side of the magnet, is fastened an auxiliary armature, so adjusted that it will not respond to the working currents but will move upward with stronger currents.

In its normal position this armature prevents a lever from answering to the gentle pull of a spring and moving forward against a contact point so connected as to shunt out the bell when the contact is established. If now a cross occurs with a light or power wire the armature moves, releasing the lever, and the protection is accomplished. It will be noticed that the form of the armature end and of the lever which rests against it are such that, on the quick upward movement of the armature, the lever is thrown back for a moment, and by this backward movement allows the armature to return in season to catch the lever before it has had time to pass under the armature. This would occur if the discharge through the magnet coil is momentary, as is the case of usual lightning discharges. Figure 10 shows this protector after it has operated by a continual dangerous current.

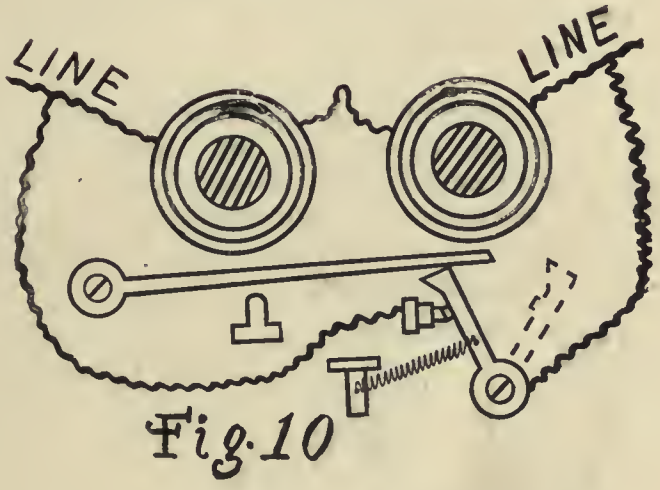


Fig. 10

A protector, resembling in construction a pile driver, has been devised by Mr. Morse, of Cambridge, Inspector of Wires for that city, and is in use on some of the city lines. The hammer is released by the magnet when excited by a heavy current. In falling it closes a shunt around the instrument and puts a ground on the circuit. It is novel in construction, but in principle like some other forms operated by the magnet.

The fourth and last class embraces "*protectors operated by heat.*" They are admirably adapted for detecting and arresting the so-called

"sneak" currents, while at the same time they are free from the objection of being affected by ordinary lightning discharges. A few forms are illustrated. Figure 11 is a thermostatic bar placed close

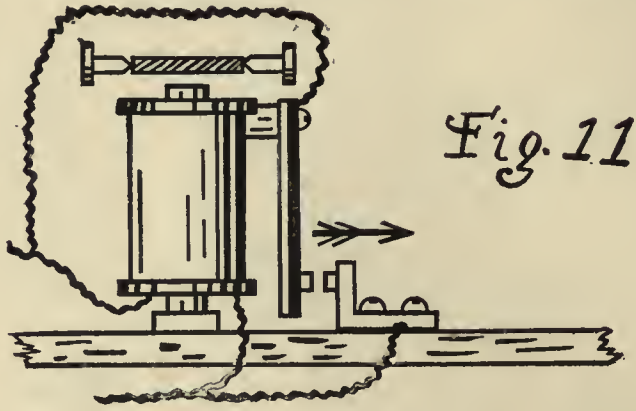


Fig. 11

beside the working magnet of the instrument to be guarded, arranged to move backward when warmed above 100°, establishing a contact which shunts the instruments. It is automatic, but too slow in operation for any other than the smallest objectionable currents. Figure 12

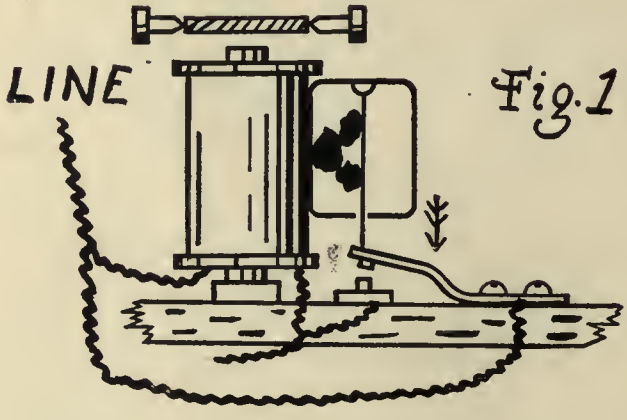


Fig. 12

shows a small metal tube or box placed against the coil of the magnet; through this box passes a thread which normally holds a flat spring away from its shunting contact. Within the metal box is also placed a little composition — not unlike that used upon the end of a friction match. The undue heat of the magnet when exposed to foreign cur-

rents ignites the thread and allows the shunting spring to operate and cut out the instrument. This reminds one of the tales of Western prairie life, where, to save the home from destruction by the oncoming prairie fire, the mother, alone and realizing the peril, catches a match and lights the grass about the cottage, beating it back and controlling it until it has diverted the prairie flames. It is fire to prevent fire.

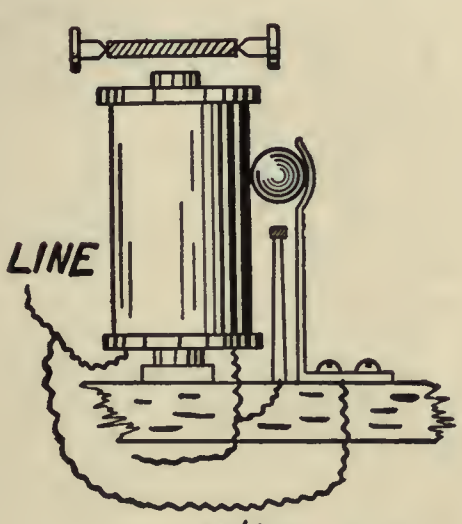


Fig. 13

Next, we have in Figures 13 and 14 two forms of the "wax ball" protector; one for shunting the instrument, the other for widely opening the line.

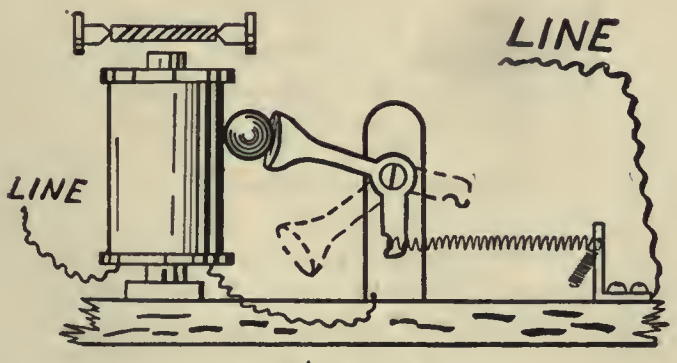


Fig. 14

A wax ball is held against the magnet coil of the instrument to be protected by the gentle pressure of a spring. Upon passage of a current of sufficient strength to warm the coil the wax softens, allowing the spring to move toward the coil until it makes contact with another spring or stud, the spring pressing the wax ball, and its contact being so wired that when there is a connection established between the two a shunt circuit is formed around the instrument. In the other form the movable wax ball support releases a spiral spring, which instead of shunting the instrument opens the main circuit widely, thus interrupting the dangerous current.

The particular advantage of this protector is sensitiveness to continuous currents which are only strong enough to endanger the finest magnet wire, while at the same time it is not troubled by lightning discharges. Other advantages are no extra magnets, resistance, or connections in the working circuit.

As already pointed out, a fuse to protect from such a current requires to be extremely sensitive, carrying not more than $\frac{1}{4}$ or $\frac{1}{3}$ of an ampère, and from its delicacy would be very frequently destroyed by discharges too feeble and of too short duration to be of any danger to the apparatus.

There are protectors in which the fuse wire is coiled upon itself for increasing its sensitiveness to the heating effect of the current. These are not simple fuses, so I have classed them with the thermostatic devices.

Having described and shown only a few of the characteristic protectors designed for telephonic as well as other delicate electrical apparatus, allow me in a few words to explain how the telephone instruments in this vicinity are protected at the present time. The aim has been to protect from the whole range of dangerous currents, both "direct" and "alternating" in character, including instantaneous disruptive discharges, long continued heavy currents, and those only just strong enough to do harm.

It has been the aim, also, to protect a metallic circuit from electrical charges which would be dangerous to persons, and which might not under certain conditions be dangerous to the apparatus. In exchanges where all these hazards exist there is employed at the telephone subscribers' stations a combination of three arresters heretofore described.

1. The wire as it enters the building is attached to a simple lead fuse six inches in length, protected in an asbestos-lined box; this fuse

will melt at six ampères of current in ten seconds, and is sufficiently sensitive to prevent danger of overheating the leading-in wires, while it will not be troubled by ordinary atmospheric or lightning discharges.

2. Near the fuse, between it and the telephone instrument, is placed an air space carbon plate arrester, operating at a difference of potential of 300 volts and upwards. This arrester is usually located within the box which incloses the fuse. See Figure 15.

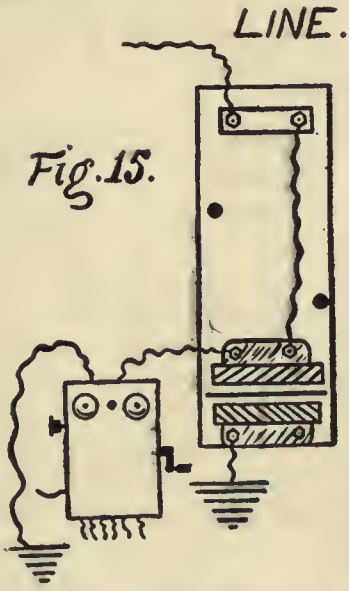


Fig. 15.

3. A thermostatic device for taking care of the weaker currents is combined with and located near the fuse, or in many cases is applied to the bell magnet, and then is known as the "wax ball" protector.

The carbon plates contain within an aperture near the inner surface a disk of fusible alloy for destroying a continuous arc, and thus preventing the danger of fire from this source. This would happen only when high potential currents of too small quantity to open the fuse were present.

The protectors in use to-day have been the subject of much study and have undergone many modifications. Dr. Hayes and Mr. White, both well known to this Society, are credited with improving much of this apparatus, particularly that employed at the central offices. F. A. Bickernell, of New York, has accomplished much in perfecting this system of protection. The central office protectors are the same in principle as those at the subscribers' stations, the fuse being located

at the outer end of the cables, one on each wire; a "heating" or thermostatic coil and an air space arrester being placed at the office end of each wire as it emerges from the cable. In the central office protector, instead of using wax as the thermostatic portion, soft solder is employed, which upon melting allows a spring to move against a contact point and thus throw on a ground between the line and the switch board. When this happens the annunciator resistance (usually 500 ohms) is shunted, and the current generally increases sufficiently to operate the fuse; if not, it is too feeble to do further harm. Figure 16 illustrates the operation of this combination.

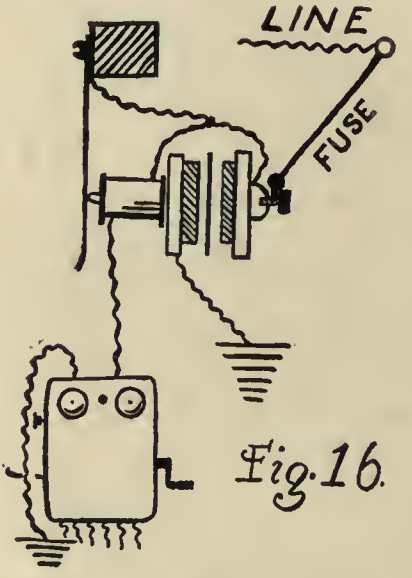


Fig. 16.

We find in practice some trouble with the air space arrester by its occasionally becoming grounded. It is not serious, however, as will be evident from the fact that in the Milk Street office, employing about 10,000 of these arresters, the average is about one trouble per day. The removal of this objection is being studied with good promise of a remedy. Observations of a large number of accidental contacts with electric light and power wires have confirmed us in the belief that as a thorough protection for telephonic apparatus this system has few, if any, equals.

[The operation of some of these protectors under widely differing conditions of current was then illustrated by tests made by use of currents of 100 to 500 volts furnished by the Institute dynamos.]

HEATING AND WORKING OF METALS BY ELECTRICITY.

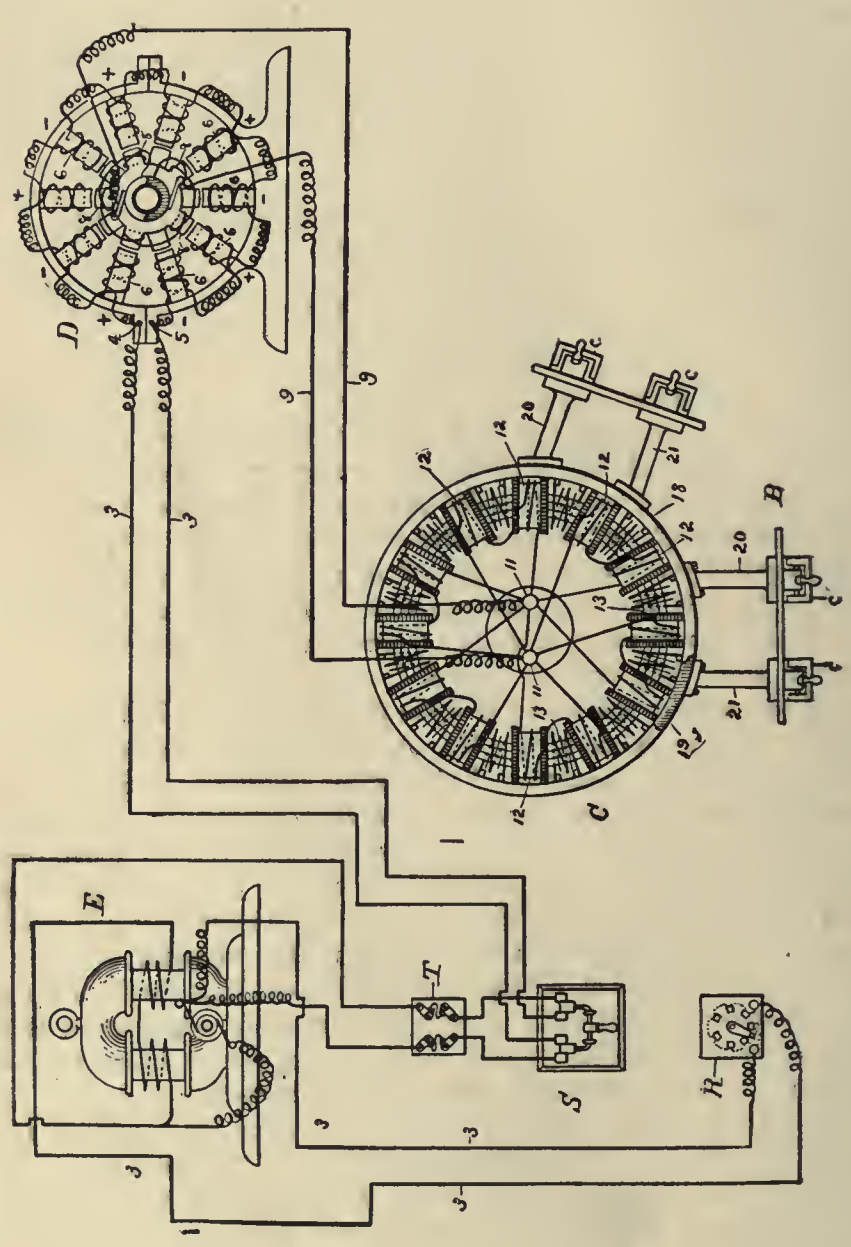
By GEORGE D. BURTON, President of the Electrical Forging Co.

Read December 22, 1892.

Accepting the invitation of the Society of Arts, I take pleasure in presenting some illustrations and description of my system of heating and working metals by electricity. By careful observation and numerous experiments I have succeeded in accomplishing some things which theory from high authority asserted could not be done. Some of you may say that you have seen in use before my time the same system that I describe. If so, I must ask you where, for I claim that this system is the first employed in working of metals successfully and economically in small, long, or large masses.

I was not the first to conceive the idea that metals could be worked by electricity. Johnson, in 1865, obtained a patent for heating and annealing wire by a current of electricity. Williams, in England, in 1881, obtained a patent for working metals by electricity, using a system of accumulators. Other inventors procured patents about 1870 for the process of working metals by a current of electricity. That the methods then employed for producing the current were too expensive was no doubt the reason why they were not successful.

The construction of our apparatus may be seen from the illustration, which shows the generator, exciter, converter, and regulator as they are connected when in general use and employed in operating this system. This metal-heating apparatus is designed to heat a bar of metal of any size or shape within the capacity of the machine; 108 cubic inches of metal have been heated at one time, with an expenditure of 47 electrical horse power. The generator shown is an alternating current generator, producing 1,600 volts and 24 ampères. Every other core of the armature of this machine is unwound — a feature which gives superior results.


D
C
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21

The current generated by this machine is transformed into a heating current of low electro-motive force and great amperage, both of which vary according to the size of the piece of metal being heated, say from 4 to 30 volts, and from 6,000 to 10,000 ampères. The dynamo "D" has its fields energized by the exciter "E," which is a shunt wound machine, exciting its own fields. This machine produces 220 volts and 6 ampères. This current is variable. The voltage and amperage vary in like proportion as 6 is to 220. The regulation is obtained by the rheostat "R." This regulating device absolutely controls the heat at the terminals of the secondaries of the transformer "C," and so minutely that a piece of metal can be raised to a white heat and maintained at that point for a great length of time with very little power.

It is to be observed that there is no obstruction introduced into the high tension circuit of this apparatus, consequently there is nothing to retard the progress of the current in its passage to the transformer, which is used for changing the current. The transformer is constructed as follows:

"E" represents the core, composed of soft iron wire. About 800 pounds is used in this apparatus in small pieces, breaking joints, as shown, and uninsulated; "12" represents the primary coils, twelve in number, and connected to the dynamo current in series, each series in pairs, and each pair in multiple arc; "14" represents the secondaries, composed of copper spirals formed by casting. There are twelve of these castings, one end of which is bolted to the ring "19," which we designate as the positive pole. The other end of the casting is bolted to the ring "18," which we term the negative pole. These rings are composed of copper, and weigh about 600 pounds each. To these rings are bolted the metal holders "22," one upon each ring. The bar or rod "B" is placed between the holders "22," and completes the circuit between the two poles. The current is then turned on, and the metal bar is heated by the inductive action of the electric current produced by the transformer.

Should there be two pieces, each 1 inch square x 3 feet long, heating at two different points about this transformer, the dynamo "D" furnishes the required current to heat said bars in $4\frac{1}{2}$ minutes, with an expenditure of 43 electrical horse power. Should it be desired to heat three pieces, each 1 x $\frac{1}{2}$ inch square x 3 feet long, heating at three different points, the dynamo would furnish the required current, which would be 52 electrical horse power for $3\frac{1}{2}$ minutes.

It will be seen that no power is wasted. The transformer calls upon the generator to furnish only sufficient power to heat the pieces held within the metal holders.

If it is necessary to fuse a piece, it might be 15 inches long and 1 inch square, and be brought to a fusing point in $2\frac{1}{2}$ minutes with an expenditure of 42 electrical horse power. As soon as the piece is melted and falls from the metal holders the circuit at the terminals of the transformer is instantly broken and all the load at the same instant is removed from the generator, except what is required to overcome friction, which is less than 1 horse power, and this all takes place without changing any of the electrical connections. The construction and arrangement of the various parts of the apparatus cause each part to act automatically and in unison with every other part of the entire system. This feature is of great importance in the successful working of metals by the use of the electric current.

Suppose in a large factory it is desired to heat twenty bars at one time, *i.e.*, to keep that number heating, constantly removing a heated bar and inserting a cold one; the automatic adjustment of our apparatus permits this, and thereby meets the customary forge or heating furnace in its every requirement.

Some of the advantages of this process which show why it commends itself in a commercial way may be stated as follows:

1. Economy in the use of the current employed to heat bars of iron or steel of any size within the capacity of the apparatus.

2. The absolute control of the electric current during the period the bars are heating.

3. Economy in time, labor, and fuel over any other method of producing heat — no time wasted in attending the forge fire.

4. Cleanliness about the workshop — no smoke, dust, or ashes to be removed; no coal to be carried to the forges; more floor space to be utilized on account of a less number of forges necessary for the same work; and absence of the intense heat in the present forge shop.

5. The fact that electric heat is the cleanest. There are no gases introduced into the metal while heating, which is quite important in many cases.

6. The evenness of the heat within and without the entire piece or bar under heat. With electric heating, results are obtained in working a piece of steel which it is impossible to obtain if the bar is heated by any other known method. A steel hammer weighing $2\frac{1}{2}$ pounds

has been formed and completed at one heating. By the usual method it is necessary to reheat the bar from three to four times in order to complete what electricity will do in one heating.

7. The greatly decreased loss by scaling. This loss under the usual method of heat is from 6 to 9 per cent. The loss in scale by electric heat varies, if the piece is heated in open air, from 2 to 3 per cent.; if under cover, so that the atmosphere is nearly excluded while the bar is heating, it is less than 2 per cent.

8. All danger from an electric shock is avoided. The heating process is conducted with the same freedom and safety as by the ordinary methods.

The question may suggest itself, Why is it essential to employ our particular system of connecting up the apparatus, and why some method other than what we show would not produce the same results? It is possible to connect up the various parts used to make this complete heating plant in a number of ways, and fair results are obtained in the use of the varied connections; but there is no other system of all that I have tried which can be operated with so little power to obtain given results as the system here described.

Having explained in a general way the methods employed, and the construction and manipulation of the electrical part of this process, I will now come to the question of cost as compared with coal, as the matter of cost is generally the first question raised by all who take an interest in this subject.

Many people believe that the cost of electric heat is enormous as compared with the cost of coal, oil, or gas. The following figures on this point, taken from the report of Mr. George L. Harvey, the well-known mechanical engineer of Chicago, which was written after spending some time in making numerous tests at our works, may be of interest:

FROM THE REPORT OF MR. GEORGE L. HARVEY.

The heating apparatus has for its object: To heat a bar of metal of any size or shape up to the full capacity of the apparatus. This is done by taking an alternating current of great force and small amperage, and transforming or converting it into a heating current of low electro-motive force and great amperage. This current is applied to the ends of a bar, or piece, to be heated, and accomplishes the result in a minute or two. The clamps attached to the transformer hold the pieces to be heated.

Three tests were made to show the horse power required to heat given sizes of bars under various conditions, from which the cost of heating the test bars could be calculated and enable one to estimate the cost of heating different bars without an actual test.

The power in the generating room was provided by a 60 horse power motor, with an

Emerson power scale so attached that the horse power delivered to the dynamo could be determined at any time. Allowance was made for friction and power required to drive the generator when the transformer was not at work.

Other tests, made previous to my work, are tabulated, to give further data on which to base my conclusions. These, I understand, were all made by parties outside of the employ of the Electrical Forging Company.

I used in making my conclusions further tests, which show that:

1. The horse power required to heat a cubic inch of material is independent of the length of bar — an unexpected result.

2. The horse power required to heat a bar decreases very rapidly as the time of heating is shortened.

This is a peculiar result, partly accounted for by the fact that the radiating surface remaining constant less heat will be lost by radiation the quicker the piece is heated, and will, therefore, require a smaller "total heat or power" to bring a given bar to a white heat.

This fact is of great practical value, as the rapidity of heating determines the value of the heating plant, for if ten minutes were required to heat a bar in a forge, and but one in this machine, you save nine tenths of the total time of not only your machines but your men.

In working pieces in a forge several reheatings are sometimes necessary, while with work heated by electricity it cannot be doubted that a less number of reheatings would be necessary. This saving of time, amounting to several per cent., must be added to the saving shown below for the electrical heating process.

3. That pieces protected from the air while heating require less heat than when exposed, which would be naturally supposed.

Below I give an estimate on a 600 H. P. plant, which according to a bid made by the Electrical Forging Company is capable of heating a billet 1 x 8 x 36, containing 288 cubic inches, in one minute to a working heat.

These figures are given by them:

Cost of plant	$22,500	
Interest, 6 per cent		$1,350.00
Repairs, 4 per cent.		900.00
Coal at 2 lbs. per H. P. per hour, at $1.25 per ton.		2,250.00
Man to run generator		900.00
Helper for heating on transformer at $1.50 per day		450.00
Total yearly cost		$5,850.00

To which must be added the power plant for running the main generators.

Engines 600 H. P. Boilers, Pumps, Heaters, Piping Stack, Shafting Power House		$39,000.00
Yearly cost:		
Interest on $39,000	$2,340.00	
Engineer	900.00	
Fireman and helper	1,800.00	
Assistant engineer	700.00	
Insurance	400.00	
Ground lease	(?)	
Repairs	2,000.00	
Total yearly cost	$8,140.00	
Power plant	5,850.00	
Total yearly cost		$13,990.00
Total daily cost	$46.66	
Or for one billet at one billet per minute for 600 minutes, viz., a day	.0778	

The cost for heating a billet by coal in a shop would be as follows:

Billet 1 x 8 x 36, 288 cu. in.; weight, 80 lbs.

[1] Coal required, 36 lbs. at $1.65 per ton . . . . . $ .0252

The time required for heating this bar will be about ten minutes longer than the electric process, which will require, as claimed, one minute. Then we must add to cost of heating by coal, ten minutes.

One smith at 30 cents	$ .05
One helper at 15 cents	.025
Total cost for heating by coal one 80 lb. billet	.10
Total cost by electricity	.0778
Gain	$ .0222
If two helpers were required, the cost of heating billets would be, by coal at $1.65	$ .125
By electricity	.078
Saving	$ .047

The electric heat acting as it does from the inside and extending outward heats a bar in a far superior manner to any external heating, even if the external heat is free from sulphur or dirt.

The central core, if required, can be heated to a liquid state, and retains heat longer than if i is heated by an open fire.

The electric method has the advantage that the heat can be applied while the bar is being forged.

The piece is always in sight, and loss of work by burning would be very rare, if not impossible.

Pieces can be heated without removal from the anvil more than a few inches.

The heat can be applied to just the part desired and no other — a great saving in power.

The loss of iron by oxidation is very slight and is given at about 2.65 per cent. when bar is heated in air, and 1.65 per cent. when heated under some protection.

It must be obvious that a quick heat will cause less oxidation than a longer one.

The temperature can be regulated to any desired degree from nothing up to melting, and held at any point as long as practical working would call for.

In tempering, bars can be brought up to a given predetermined heat by setting the regulator in a certain position for the heating of a number of pieces as long as the power remains constant.

There are no working parts about the transformer, which should last for a long time without any repairs.

The heating of a piece is even through its entire length, which is very difficult with ordinary heating processes, and would for some work be very valuable.

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[1] The price of blacksmith coal, $1.65, used in the estimate is as much lower than the market as $1.25 per ton for slack coal to be used under the boilers, so that the estimates for coal and the comparison are on the same basis.

The fifteen power tests given in the table below were taken in October last by Mr. F. H. Crane, mechanical engineer of the Emerson Power Scale Company. It will be seen that the percentage of power used in heating brass is much less than for heating iron or steel, but the time required to heat brass is longer than that required to heat iron or steel, so that the horse power necessary to heat a cubic inch of brass per minute figures about the same in the final result. This also applies in the heating of copper, and shows that the cost of heating iron, brass, copper, or steel by electricity is about the same.

Power Tests on 50 H. P. Electrical Heating Apparatus of the Electrical Forging Co., Boston, by F. H. Crane, S.B., mechanical engineer, Emerson Power Scale Co., Boston.

No. of Pieces.	Kind of Metal.	Size of Each Piece Heated to a Forging Heat.	Cubic Inches.	Time Heating. M. S.	Average H. P.	H. P. per Cubic Inch per Minute.
2	Iron.	1 x ½ in. x 4 ft. long . . .	48	3.20	51.5	3.75
4	"	½ x ½ in. x 3 ft. long . . .	36	2.	58.8	3.27
4	"	1 x ½ in. x 3 ft. long . . .	72	4.	52.7	2.93
2	Steel.	1 x 1 in. x 3 ft. 1 in. long . .	74	5.	44.8	3.02
3	"	1 x 1 in. x 3 ft. long . . .	108	8.	47.	3.48
3	Iron.	1 x ½ in. x 3 ft. long . . .	54	3 30	52.4	3.38
3	"	1 x ½ in. x 4 ft. long . . .	72	4.	53.1	2.95
4	"	1 x ½ in. x 4 ft. long . . .	96	6.	54.8	3.43
4	Steel.	1 x ⅜ in. x 4 ft. long . . .	72	4.	58.3	3.24
2	Iron.	1 x ¾ in. x 4 ft. long . . .	72	5.	49.9	3.44
4	"	1 x ⅜ in. x 3 ft. long . . .	54	2.45	58.3	2.97
4	"	1 x ⅜ in. x 3 ft. long . . .	54	2.40	57.	2.82
4	"	½ x ½ in. x 3½ ft. long . . .	42	2.30	52.4	3.09
4	"	1 x ⅜ in. x 3½ ft. long . . .	63	3.20	53.8	2.84
1	Hard Brass.	1 x 1 in. x 2 ft. 8 in. long . .	32	3.30	34.2	3.74

ON THE DISSOCIATION OF HYDROGEN IONS FROM ACID SALTS.

By ARTHUR A. NOYES, Ph.D.

Received January 27, 1893.

The chemical activity of an acid, or the readiness with which it causes and enters into chemical reactions, depends, according to the electrolytic dissociation theory, on the extent of its dissociation into ions; and the measurement of the rates at which a reaction takes place under the influence of various acids is one of the methods of determining their relative dissociation. The reaction most commonly employed for this purpose is the inversion of cane sugar, which takes place only in the presence of hydrogen ions and at a rate nearly proportional to their concentration. In an article recently published Trevor[1] has shown that this method can also be used at 100° for measuring very small degrees of dissociation, and has applied it to the determination of the number of hydrogen ions dissociated from acid salts. His results are of special interest, as they furnish the only data of the kind which we possess; for the measurement of the electrical conductivity is of no assistance in this case since so many different kinds of ions are present. In connection with some solubility experiments with the acid potassium tartrate my attention was especially directed to this article, and I desire here to offer an explanation of some of the general results arrived at by the author.

I refer in the first place to the change of the dissociation into hydrogen ions with the dilution. In the case of weak acids Ostwald and others have found the dissociation nearly proportional to the square root of the volume in which one molecular weight is dissolved,

[1] Zeitschrift für physikalische Chemie, 10, 321.

and this result is a necessary deduction from the law of chemical mass action. In the case of acid salts of weak acids, however, Trevor finds that the dissociation increases in the same proportion as the volume itself; in other words, that the quantity of hydrogen ions in the solution is independent of the dissolved quantity of the acid salt. It appears to me of some importance to show that this remarkable result, instead of being a contradiction, is a consequence of the law of mass action, and especially so, as it offers an interesting and experimentally confirmed application of that law to a rather complicated case of equilibrium.

The cause of this difference in the behavior of weak acids and their acid salts lies in the fact that, while in the case of the former the two ions are necessarily produced in equal quantities, this is by no means true in the case of acid salts, as will be evident from what follows. For there exist in the solution of an acid salt MHA seven different kinds of molecules, namely: MHA, $\overset{+}{M}$, $\overset{-}{HA}$, $\overset{+}{H}$, $\overset{=}{A}$, H_2A, and M_2A. In dilute solution — the strongest investigated by Trevor was $\frac{1}{32}$ normal — the quantities of the undissociated salts MHA and M_2A, are very small and can be neglected. The quantity of the metal ion M does not come into consideration. Between the other quantities, according to the law of mass action, the following relations exist, where the symbols stand for the molecular quantities in the unit of volume of the substances or ions represented by them, and k and c are dissociation constants:

$$c \times \overline{HA} = H \times A \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$k \times \overline{H_2A} = H \times \overline{HA} \quad . \quad . \quad . \quad . \quad (2)$$

Furthermore, as the negative ions A are produced according to the chemical reaction: $2HA = H_2A + A$, as well as by the direct dissociation $HA = H + A$, we have the equation:

$$A = H + \overline{H_2A} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and finally calling m the total number of molecules in the solution, or the concentration of the salt:

$$m = \overline{HA} + \overline{H_2A} + A \quad . \quad . \quad . \quad . \quad (4)$$

where, as above said, the quantities $\overline{MHA}$ and $\overline{M_2A}$ are neglected.

From (1) and combination of (3) and (4) follows:

$$\overline{HA} = \frac{H \times A}{c} = m + H - 2A \quad \text{or}$$

$$H \times A = cm + cH - 2cA \quad . \quad . \quad . \quad . \quad . \quad (5)$$

From (1) and from combination of (2) and (3) follows:

$$\overline{HA} = \frac{H \times A}{c} = \frac{kA - kH}{H} \quad \text{or}$$

$$A = \frac{ckH}{ck - H^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

If this value is substituted in equation (5) and the expression reduced we get

$$ck = \frac{(k + m + H)\, H^2}{m - H} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

According to Trevor's experiments, however, the ratio of the quantity of hydrogen ions to the total quantity of the salt m is very small — in almost all cases less than one per cent. Formula (7) can therefore be written without sensible error:

$$ck = \frac{(k + m)\, H^2}{m} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

or, putting $\frac{H}{m} = d$, the dissociation,

$$ck = (k + m)\, md^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The constant k in these equations is the affinity constant of the free acid, and is known from the measurements of the electrical conductivity.

It is now clear, when the acid is so weak that its dissociation constant k can be neglected in comparison with the concentration m, that formulas (8) and (9) reduce to:

$$H = md = \sqrt{kc} = \text{a constant.}$$

That is, the law of mass action requires in the case of an acid salt of a sufficiently weak acid that the quantity of hydrogen ions be independent of the concentration, or, in other words, that the dissociation increase proportionately to the dilution.

Let us consider now Trevor's results. The acid salts whose dissociation he found to increase proportionately to the dilution are given in the following table, together with their percentage dissociation:

Acid sodium salt of	$m = 0.0312$	$m = 0.0156$	$m = 0.0078$	$m = 0.0039$	Constant of the acids.
Suberic acid	0.03	0.05	0.10	0.20	0.0030
Adipic acid	0.04	0.07	....	....	0.0037
Glutaric acid	0.04	0.07	0.13	....	0.0047
Succinic acid	0.05	0.08	0.14	0.33	0.0067
Pyrotartaric acid	0.04	0.08	0.15	....	0.0086

In the last column the affinity constants (k) of the free acids are given as determined by Ostwald. These values are a hundred times greater than the true dissociation constants. These latter are therefore entirely negligible even in comparison with the smallest value of the concentration m; for example, 0.000067 in comparison with 0.0039; the proportionality between dissociation and dilution was therefore to be expected.

In the case of the salts of the stronger acids Trevor found that the dissociation increases more slowly than the dilution. This increase should take place theoretically according to formula (8). An exact agreement of the observed and calculated values is, however, not to be expected, for both Ostwald[1] and Trevor[2] have established the fact that the ratio of the velocity of inversion to the quantity of hydrogen ions decreases with increasing dilution. I give some examples to show that an approximate agreement nevertheless exists. This, in the case of oxalic acid — where, moreover, the dissociation is large — is quite satisfactory; and in all cases the small deviations lie in the expected direction. The calculated values of the dissociation in the following table are obtained by calculating by means of formula (8) (formula (7) in case of oxalic acid) from the values inclosed in parentheses those for the other concentrations.

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[1] Journal für praktische Chemie, (2) 31, 312.

[2] Zeitschrift für physikalische Chemie, 10, 339.

Acid sodium salt of		Value of the Dissociation for			
		$v = 32$	$v = 64$	$v = 128$	$v = 256$
Ortho-phthalic .	found . . .	0.19	0.34	0.62	....
	calc.	0.17	(0.34)	0.66	....
Meta-phthalic. .	found . . .	....	....	0.69	1.23
	calc.	....	....	0.63	(1.23)
Citraconic . . .	found . . .	0.12	0.21	0.33	0.59
	calc.	0.09	0.18	(0.33)	0.58
Oxalic	found . . .	....	2.92	4.37	6.13
	calc.	....	3.00	(4.37)	6.24

In regard to a second point considered in Trevor's article I would like to say a few words. In comparing the dissociation of the acid salt with that of the corresponding acid only the quantity of the hydrogen ions is considered. Now, while in the case of an *acid* the two ions are necessarily produced in equal quantities, this is not true in the case of an acid *salt*, but the acid ion $\bar{\bar{A}}$ is always present in greater quantity, and in so much greater quantity the weaker the acid. (Compare formula (3).) It is evident from formula (8) that the quantity of hydrogen ions depends not only on the dissociation constant c of the acid ion $\overline{HA}$, but also on the constant k of the acid itself. I have therefore calculated from Trevor's experiments the values of the true dissociation constant c for the dilution $v = 64$, and these values are given in the following tables :

HOMOLOGOUS ACIDS.

	$k \cdot 10^6$	$c \cdot 10^6$	$100\,d$.
Suberic	30	2.1	0.05
Adipic	37	3.2	0.07
Glutaric	48	2.5	0.07
Succinic	67	2.3	0.08
Pyrotartaric	86	1.8	0.08
Malonic	1,580	0.75	0.21
Oxalic	100,000	16.	2.92

If only the relative quantities of hydrogen ions (given in the last column) are considered we should conclude with Trevor that the dissociation of the free acids and that of their acid salts varies in the same direction. This behavior would be, however, inconsistent with the hypothesis of Ostwald[1] on the cause of the weakness of the acid salts, according to which the negative charge of the first carboxyl group prevents the dissociation of the hydrogen from the second; for this should take place with greater difficulty the nearer the two carboxyls are to each other. By comparing the dissociation *constants c*, however, this requirement of the hypothesis is seen to be in general fulfilled; for while *k* on account of the greater proximity of the carboxyls increases, *c* regularly decreases for the same reason. Only oxalic acid, the extreme member of the series, forms an important exception.

ISOMERIC ACIDS.

	$k \cdot 10^6$	$c \cdot 10^6$	$100\,d$.
o-phthalic	1,210.	2.2	0.62
m-phthalic	290.	10.	0.69
Maleic	11,700.	0.55	0.39
Fumaric	930.	20.	0.85
Citraconic	3,400.	0.39	0.21
Mesaconic	790.	6.3	0.44

In the case of these isomeric acids we find again the same relations between the dissociation constants of the acids and those of their acid salts; only in this case the differences in the latter are so great that the relative dissociation of the salts is evident from the quantity of hydrogen ions alone.

MALONIC ACID DERIVATIVES.

	$k \cdot 10^6$	$c \cdot 10^6$	$100\,d$.
Malonic	1,580.	0.75	0.21
Ethylmalonic	1,270.	0.35	0.13
Dimethylmalonic	770.	0.27	0.09

[1] Zeitschrift für physikalische Chemie, 9, 558.

The conditions here are exactly the reverse of those illustrated by the previous tables — a greater dissociation of the acid corresponds to a greater dissociation of the salt. Exactly the same thing is found on comparing maleic and citraconic acids, or also fumaric and mesaconic acids. (See the previous table.) All these cases are, however, different in principle from those before considered; for the greater dissociation of the one acid is here caused not by a nearer position of the carboxyls to one another, but by the introduction of more negative groups; and this latter increases, of course, also the dissociation of the second hydrogen ion.

The two following principles can then be stated as corollaries from Ostwald's hypothesis.

1. The dissociation constant of a dibasic acid is greater, and that of its acid salt is smaller, the nearer the carboxyl groups are to each other.

2. The introduction of new groups into a dibasic acid influences the dissociation constants of the acid and its acid salt in the same direction.

These principles are confirmed, as we have seen, by Trevor's experiments.

THE STUDY OF BACTERIA IN DRINKING WATER.

By GEORGE W. FULLER, S.B., Bacteriologist in Charge, Lawrence Experiment Station, State Board of Health of Massachusetts.

Received February 8, 1893.

The bacteriology of water[1] has advanced with an increased impetus during the past few years, since it has been established that the specific germs of several diseases are able to live for some time in potable water, and that the death rate of cities and towns in regard to these diseases is, to a large extent, dependent upon the quality of the water supply.

As is well known, the number of bacteria in water and in other materials is determined by the use of the gelatine-peptone plate method devised, in 1881, by Koch. A known amount of water is mixed with a tube of properly prepared melted gelatine and poured into a glass dish. The gelatine, as well as all apparatus with which the water comes in contact, must have been previously sterilized. The gelatine quickly solidifies and holds in isolation each individual bacterium, which multiplies rapidly in this rich food material, and after two to three days at a temperature of 20° C. forms a "colony." These colonies are readily counted by the aid of a hand lens.

From the outset it was more or less recognized that this method was not a perfect one, and that some species of bacteria are not able to develop in this culture medium. The bacillus of tuberculosis fails to grow in nutrient gelatine, and the nitrifying organisms, which play such an important rôle in the economy of nature by converting decaying organic matter to harmless mineral matter, entirely escaped identification for many years, on account of their inability to develop in this medium. A large majority of these minute plants require an alkaline food material, and, furthermore, it has been found that different

[1] For the literature and a summary of our present knowledge upon this subject see Report of Profs. P. F. Frankland and Marshall Ward, Proceedings of the Royal Society, Vol. LI, No. 310.

species of bacteria in water require different degrees of alkalinity. Experienced bacteriologists[1] agree that, in order to make bacterial water analyses of value for comparison, the culture medium shall be of a uniform degree of alkalinity (the optimum degree for maximum development), and that the conditions in regard to the composition of the nutrient gelatine, the temperature, and period of cultivation shall be as nearly parallel as possible. The following table shows the variableness of the number of bacteria in the same sample from the use of gelatine of different degrees of alkalinity, and with all other conditions the same. Number A was acid; the others were alkaline and in regularly increasing amounts. The numbers express the bacteria in a cubic centimeter.

	A.	B.	C.	D.	E.
Lawrence City water . . .	3	18	160	240	606
Lawrence sewage	146,000	517,000	770,000	1,526,000	910,000

In addition to the many improvements which have been made in the general methods and manipulation, special methods and media have been devised for the study of some of the most important species of bacteria.

After the introduction of this quantitative method numerous determinations were made of the bacterial contents of water. It was found that many surface waters were very rich in bacterial life, while spring and deep well waters were, as a rule, comparatively free. In a general way it was learned that the number of bacteria in a water was proportional to the amount of organic matter present which could serve them as a food material. Enthusiastic bacteriologists were not slow to set up arbitrary standards for the purity of water, based on the results of their determinations, and to express the opinion[2] "that chemical analyses were retained and practiced only on the ground of thoughtless tradition."

The promoters of this doctrine met a severe setback in 1885, when it was discovered[3] that bacteria are able to live upon an ex-

[1] Reinsch, Centralblatt für Bakteriologie, 1891, 10, 415. Annual Report of Massachusetts State Board of Health for 1891, p. 620. Dahmen, Chemiker-Zeitung, 1892, No. 49.

[2] Fraenkel, Grundriss der Bakterienkunde, Dritte Auflage, S. 487.

[3] Meade Bolton, Zeitschrift für Hygiene, 1, 76.

tremely small amount of food material. It was even claimed at that time that certain species of bacteria are able to multiply in distilled water. The manipulation in these experiments was not above criticism, but in the light of our present knowledge it is definitely known that some kinds of bacteria grow on very small amounts of organic matter. It has been found that several species of bacteria present in the water of the Merrimack River are capable of growth in a solution containing one part of peptone in one hundred million parts of redistilled filtered water. From this it will be readily understood that some bacteria multiply to a considerable extent in water containing the merest trace of harmless organic matter of a vegetable origin; and that from this time forth cautious sanitarians have regarded the bacteriological analysis of water as an important supplement to, but by no means a substitute for, chemical analysis.

Modern hygiene demands that a drinking water shall be free from all disease-producing germs. The old conventional standard that a good water ought not to contain more than two hundred bacteria per cubic centimeter is evidently untrustworthy, because, as has been many times pointed out, it is perfectly possible that a water might contain five thousand bacteria of a harmless nature, and not be dangerous for drinking, while another water might have ten bacteria in a cubic centimeter among which were germs of typhoid fever or of Asiatic cholera. The greatest service which the bacterial analyst of water can render hygiene is to be able to quickly and accurately determine whether a given water contains any pathogenic bacteria. Unfortunately, with the methods at our present command, this problem is extremely difficult, and requires several days and possibly weeks of careful work before it can be solved, and then, in many cases, the results are associated with more or less doubt. Furthermore, in many cases the water is not submitted to the bacteriologist until the epidemic is practically over, and the course of the disease is therefore very little, and possibly not at all, affected by the bacteriological investigations. It is accordingly necessary for the health of the consumers to condemn all drinking water which contains an appreciable amount of organic matter of an animal origin which is usually associated to a certain extent with disease germs. The fitness of a water for drinking purposes should be determined by a careful inspection of its source in addition to the bacterial and chemical analyses.

In selecting a public water supply many cities and towns are

obliged to choose a water which is not above suspicion as a carrier of disease germs. The question then arises, How can such a water be purified? In many cases this can best be done by means of filtration through sand. This method is successfully used by many of the large European cities, and its day in America is just dawning. In determining the efficiency of filtration and in obtaining useful data for the construction and manipulation of filters bacteriology plays an important part. In no other field are the bacterial methods so well adapted to the needs of the problem. Here the possible variations in bacterial results arising from certain defects in the method are offset when, under parallel conditions, the number of bacteria in the water are determined before and after passage through the filter, and a true idea of the degree of bacterial purification is obtained. The services rendered by bacteriology to hygiene in this direction are of a most practicable as well as highly important nature.

Another fruitful field of bacterial investigation is the determination of the vitality of pathogenic bacteria in drinking water. It is claimed, for instance, that the diphtheria bacillus[1] does not live in water, while the bacilli of typhoid fever and of Asiatic cholera live in diminished numbers for several weeks and possibly months. The germ of typhoid fever has been found to live for twenty-four days[2] in the water of the Merrimack River at Lawrence. To study this problem under conditions parallel to those which occur in nature is extremely difficult. Fortunately for the public health disease germs very rarely occur in water in numbers sufficiently great to enable them to be readily recognized among the harmless species of water bacteria. It thus becomes necessary to inoculate the water with the germs to be studied; and with them, in many cases, is introduced considerable organic matter which serves them as a food material. The nature of the associated species of water bacteria, the food material, temperature, aëration, relation to light, etc., differ much in different experiments, and doubtless go far to explain the wide divergences in the results of several investigators.

The following method of procedure has been found to serve well for the inoculation of water: The bacteria are grown for four days at 20° C., or for two days at 37.5° C., in a solution containing one tenth per cent. peptone and two tenths per cent. glucose in city water

[1] Thorne. Diphtheria: Its Natural History and Prevention, p. 101.

[2] These and following data appeared in the State Board of Health Report for 1891.

which contains phosphates and other desirable inorganic compounds. The water is infected by introducing this solution in the proportion of one part in ten thousand. The amount of organic matter added to the water with the bacteria has been found to be practically within the limits of variation of the organic matter of the water of the Merrimack River.

After a water is properly inoculated with disease germs it is no easy task, in many cases, to differentiate the bacteria under consideration from those regularly present in the water. It is first necessary to obtain a good general knowledge of the specific characteristics of the water species, and from these data devise means of differentiation. The prominent part in the development of bacteriology which has been taken by harmless water bacteria is remarkable. Bacteriology was first applied to the study of pathogenic bacteria, and methods for their differentiation were found. After our knowledge was extended to the bacteria of water it was learned that some of them fulfilled all the laboratory requirements for the identification of disease germs. Thus in the case of the bacillus of typhoid fever it was thought for several years that the characteristic invisible growth upon sterilized potato was sufficient to differentiate it from all other species. It is now known that at least five species of water bacteria satisfy this and other laboratory conditions, and new methods of diagnosis have been accordingly devised. The comparative study of the different kinds of bacteria in water, indeed, offers a very fruitful field of investigation to well-trained bacteriologists.

In making a thorough hygienic study of the water of the Merrimack River at Lawrence, one of the first steps was to become familiar with the prevailing species of bacteria. Side by side with this study the problem of bacterial purification by sedimentation and passage through iron pipes was also investigated. Several times each month bacterial samples of water were taken from different parts of the Lawrence Water Works as follows:

1. From the force main at the pumping station as the water comes direct from the river.

2. After the water had passed through the reservoir, which has a capacity of about two weeks' consumption.

3. From a tap at the City Hall, which is about one and a half miles distant from the reservoir.

4. From a tap at the experiment station, which is about two and a half miles distant from the reservoir.

TABLE OF MONTHLY AVERAGES OF THE NUMBER OF BACTERIA IN A CUBIC CENTIMETER OF WATER FROM DIFFERENT PARTS OF THE LAWRENCE WATER WORKS.

1891.	Pumping Station.	Reservoir.	City Hall Tap.	Experiment Station Tap.
January	2,872	713	347	72
February	1,387	224	92	34
March	890	222	98	33
April	770	390	190	62
May	4,030	242	68	49
June	3,690	192	50	95
July	5,030	507	219	188
August	1,920	317	521	181
September	12,150	262	166	144
October	12,550	888	975	356
November	3,576	408	300	122
December	11,440	2,384	1,215	401
Averages	5,025	562	353	145

From this table it will be seen that the number of bacteria in the water drawn from service pipes is much smaller than in the river water, and that this decrease takes place chiefly in the reservoir, due largely to sedimentation, lack of suitable food material, and influence of light. The average amount of bacterial purification for the year 1891 of water at different places was as follows:

TABLE SHOWING THE PERCENTAGE OF BACTERIA IN THE WATER OF THE MERRIMACK RIVER WHICH WERE REMOVED BY PASSAGE THROUGH THE RESERVOIR AND PIPES OF THE LAWRENCE WATER WORKS.

Water after leaving the reservoir	89	per cent. of bacteria removed
Water from tap at City Hall	93	" " "
Water from tap at experiment station	97	" " "

This table does not give a true idea of the relative purity of the service water, because some of the river water passes through the

reservoir before purification takes place. This amount varies, among other things, with the temperature of the river water compared with that of the water which it meets in the reservoir. If the entire supply remained in the reservoir for two weeks the purification would doubtless be greater and the danger in using it for a drinking water diminished to a certain extent. It is not advisable, however, to depend upon purification by storage alone now that it has been determined that a practically complete bacterial purification of the water can be effected by filtration through sand.

In order to study more closely the seasonal effects upon the bacteria and the life history and specific characteristics of the different species, the number present of each of the prevailing species of bacteria in the different samples was determined. The quantitative determination of the prevailing species of bacteria in water is not known to have been made elsewhere. The method practiced is as follows:

All of the colonies on a gelatine plate are carefully examined and an estimation made of the number of each kind presenting marked characteristics. This places together in groups those kinds superficially resembling one another. One or more colonies of each group are "fished," and ten or more colonies possessing no marked characteristics on the gelatine (including many non-liquefying colonies lying beneath the surface) are taken at random; and in case of less than ten such colonies all are taken. To facilitate the separations dilutions are made of those waters containing high numbers of bacteria. A pure culture is obtained from each colony that is fished, and their identity established by the tests regularly employed by bacteriologists. After this is done the number present of those species possessing marked characteristics on the gelatine plate is easily calculated, and of the remainder each species is given its aliquot portion.

From a classification of the data upon the specific characteristics of the different water bacteria, a scheme of procedure can be developed which will enable pathogenic bacteria to be most readily and accurately differentiated. The life histories of the prevailing species of bacteria in water are in themselves of interest and importance, and are of still greater value for comparison. When laboratory experiments upon the life history of a disease germ in water are made side by side with those upon a prevailing species of bacteria in the given water, and the life history of which, under conditions actually found in nature, is already known, an excellent check is obtained upon the experimental conditions.

This investigation showed that *B. coli communis*, the leading species of bacteria in human fæces, formed thirty-nine per cent. during the months of November and December, 1891, of the bacteria in the water of the Merrimack River at Lawrence. Its life history in the water, after passing through the reservoir and service pipes, indicated it to be a very persistent organism. It has accordingly been used to advantage for laboratory experiments. In virtue of its origin, moreover, its presence in a water is a ready indication of contamination of animal nature and of danger from its use as a drinking water.

MEASUREMENT OF STEAM BY THE FLOW THROUGH AN ORIFICE.

By E. F. MILLER, S.B., and C. A. READ, S.B.

Received February 9, 1893.

In the apparatus shown in Figure 1, page 359, FF are two pieces of 2″ pipe each about 16″ long, having a 2″ pipe flange on one end and a reducing coupling on the other. There is a gauge B and a drip D on the entrance side, and a gauge C and thermometer cup E on the exit side. At A between the flanges there is a brass plate with an orifice through which the steam to be measured flows. If the pressure at C is much lower than that at B the steam on the exit side of the orifice is apt to be superheated, the number of degrees of superheat being the difference between the thermometer reading at E and the temperature of steam corresponding to the pressure C. Knowing this and the pressure C we can calculate the British Thermal Units per pound of steam supplied to the machine tested. This machine when made of 2″ pipe is suitable for amounts up to 250 pounds per hour with 110 pounds gauge pressure at B. It is not advisable to have the orifice in the plate A over $\frac{1}{2}$″ diameter if FF is of 2″ pipe; but if FF is made larger in diameter, larger orifices can be used. By having a number of orifice plates A with different sized holes the weight of steam delivered can be varied, as can also the back pressure C. The orifice plates are brass disks 5″ diameter and $\frac{1}{4}$″ thick, such as are kept in stock at hardware stores. The orifices were bored out in a speed lathe, no pains being taken to get any special diameter. The diameters were obtained by fitting arbors to the holes and then calipering the arbors with a micrometer caliper reading to one ten-thousandth of an inch. The entrance side of the orifice is rounded with a smooth curve tangent to the plate and to the straight part of the orifice.

Two pieces of apparatus like that described above, also one of 3″ pipe, were made and tested in the Engineering Laboratories. The tests, many of which formed part of the fourth year laboratory work,

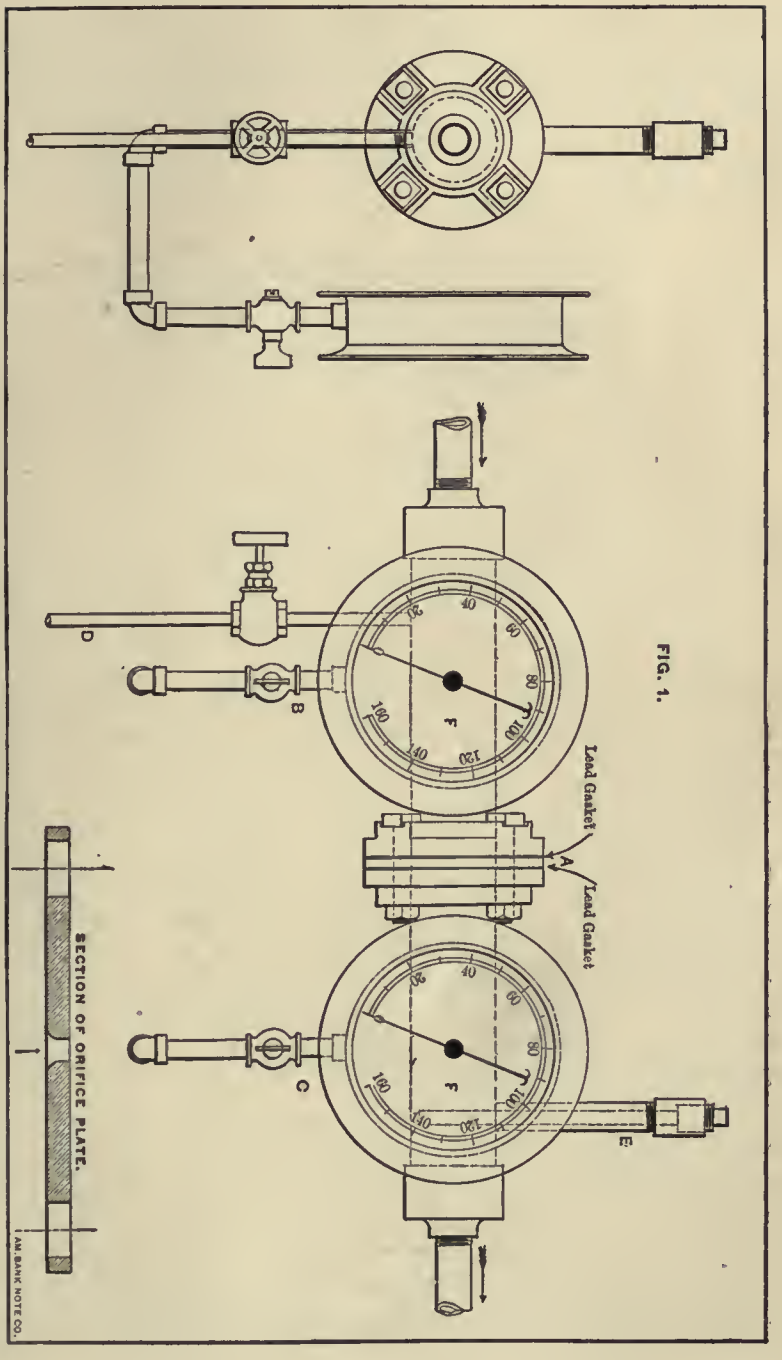
D
B
F
Lead Gasket
A
Lead Gasket
C
E
SECTION OF ORIFICE PLATE.
AM. BANK NOTE CO.

FIG. 1.

were carefully made. Steam gauges B and C were tested by comparison with an open mercury column. The steam after passing through the apparatus was condensed in a Wheeler condenser, which was repeatedly tested for leaks and found to be perfectly tight. The calculated amount was obtained by applying Napier's formulæ given below:

$$P_1 = \text{or} > \tfrac{5}{8} P_a \qquad G = F \frac{P_1}{70}$$

$$P_1 < \tfrac{5}{8} P_a \qquad G = F \frac{P_a}{42} \left\{ \frac{3 (P_1 - P_a)}{2 P_a} \right\}^{\frac{1}{2}}$$

P_1 is the absolute pressure on entrance side.
P_a is the absolute pressure on exit side.
F is the area of the orifice in square inches.
G is the weight in pounds per second flowing through.

In Table I tests 1 to 15 inclusive were made with the 2″ apparatus. The orifice used was .2285″ diameter. These tests are arranged with reference to the ratio of the absolute pressures — Column 6. The coefficient given in Column 9 is the number by which the calculated amount, given in Column 8, must be multiplied to get the correct amount determined by experiment — Column 7. This coefficient gradually increases as the difference of pressures decreases. With a ratio of pressures equal to .9, the disagreement of Napier's formulæ appears to be about 5 per cent., while with a ratio equal to .5, the disagreement is about 2 per cent.

Tests 16 to 26 inclusive were on the 3″ apparatus with an orifice .4136″ diameter. In these the coefficients increase as the difference of pressures decreases, but in every case the coefficients are larger than for the same ratio of pressures in tests 1 to 16. Tests 27 to 32 are a few scattering tests with different boiler pressures. After having determined for an orifice coefficients for different ratios of pressures, the boiler pressure being very nearly constant, this apparatus can be used with a fair degree of accuracy.

It is a very convenient apparatus for finding the weight of steam used by inspirators, ejectors, pulsometers, and other apparatus of the same sort, where steam is in contact and mingles with the water pumped, rendering the use of a condenser impracticable. The cost of the apparatus, not including the gauges, did not exceed three dollars.

Besides these tests a number were made on a similar apparatus, a description of which, together with some results, can be found in

Volume XI of Transactions of the American Society of Mechanical Engineers. In this apparatus the chambers FF are about 5″ diameter and 24″ long. An orifice .25″ diameter and $2\frac{1}{2}$″ long was used (Figure 2). By means of a small hole entering, at right angles, the straight

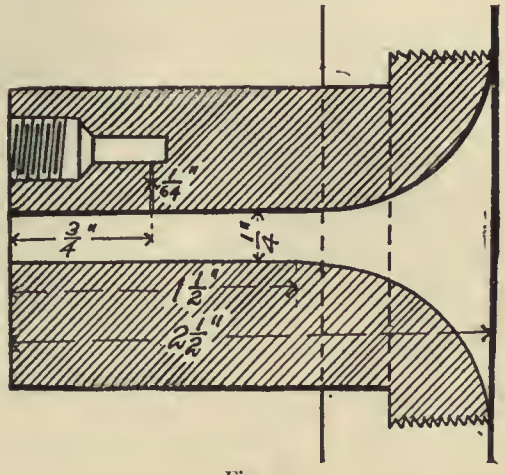

Fig. 2.

part of the orifice, and a connecting pipe running through a stuffing box on the outside, the pressure in the orifice was measured. As the chamber is large compared with the orifice it is assumed that the velocity in the chamber is zero.

For an adiabatic flow through the orifice we have the formulæ (see "Thermodynamics of Steam Engine." — *Peabody*):

$$A\frac{w^2}{2g} = x_1 r_1 + q_1 - x_2 r_2 - q_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{x_1 r_1}{T_1} + \Theta_1 = \frac{x_2 r_2}{T_2} + \Theta_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$G = \frac{f\,w}{x_2 u_2 + .016} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

A = heat equivalent of one foot pound.
w = velocity in feet per second of steam in orifice.
$2g$ = 64.32.
x_1 = quality of entering steam as determined by a calorimeter test .989 throughout all these tests.
r_1 = latent heat at absolute entrance pressure.
T_1 = absolute temperature of steam at entrance.
Θ_1 = entropy of liquid at entrance pressure.
q_1 = heat of liquid at entrance pressure.
f = area of orifice in square feet.

x_2, r_2, T_2, and Θ_2 all refer to the above at absolute orifice pressure.

$u_2 = S_2 - .016$ = volume of a pound of steam at orifice pressure, minus volume of a pound of water.

G = weight in pounds per second flowing through.

x_1 being known, x_2 is obtained from equation (2), and this value substituted in (1) enables us to solve for w. In equation (3) the numerator is the number of cubic feet per second, and the denominator is the number of cubic feet in one pound of the mixture of steam and water in the orifice.

In Table II the calculated weight was worked by the thermodynamic formulæ and also by Napier's formulæ. Column 8 gives the coefficients for Napier's formulæ, and Column 10 those for the thermodynamic equation. These coefficients are nearly alike and all are less than one, while in the first table the coefficients were all more than one. This difference is probably due to the greater friction in the longer orifice. The orifices soon wear rough, and must be trued and polished frequently.

TABLE NO. I.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
No. of Test.	Boiler Pressure by Gauge.	Back Pressure by Gauge.	Boiler Pressure Absolute.	Back Pressure Absolute.	Ratio of Absolute Pressures.	Steam by Experiment, lbs.	Steam by Equation, lbs.	Coefficient.	Diameter of Orifice, ins.
1	108.0	41.2	122.8	56.0	.500	264	259	1.019	.2285
2	108.3	51.2	123.1	66.0	.536	265	259	1.023	"
3	107.5	55.5	122.7	70.3	.573	265	259	1.025	"
4	107.6	60.0	122.4	75.8	.619	262	256	1.024	"
5	108.5	58.7	123.3	73.5	.596	266	260	1.025	"
6	108.3	59.6	123.1	74.7	.607	266	259	1.025	"
7	106.6	61.9	121.5	76.8	.632	261	252	1.037	"
8	107.8	64.1	122.7	79.2	.645	262	252	1.040	"
9	108.0	68.7	122.9	83.6	.680	257	247	1.040	"
10	108.4	70.8	123.1	85.5	.695	251	244	1.027	"
11	108.4	81.5	123.3	96.4	.772	227	219	1.036	"
12	108.6	86.4	123.5	101.3	.820	215	204	1.054	"
13	110.2	92.8	125.2	107.8	.861	194	186	1.043	"
14	109.4	95.5	124.3	110.4	.888	178	169	1.058	"
15	110.5	100.9	125.5	115.9	.924	151	143	1.056	"
16	92.0	7.2	107.0	22.0	.206	770	739	1.055	.4136
17	96.3	8.3	111.2	23.2	.209	796	768	1.035	"
18	45.5	2.9	60.4	17.8	.294	441	417	1.057	"
19	97.1	30.6	112.0	45.5	.407	808	774	1.044	"
20	95.5	40.2	110.4	58.1	.526	795	763	1.042	"
21	95.7	49.8	110.4	64.5	.584	798	763	1.046	"
22	95.8	54.9	110.6	69.7	.630	793	754	1.052	"
23	93.8	60.2	108.6	75.0	.691	762	708	1.076	"
24	93.2	65.0	108.2	80.0	.749	734	700	1.094	"
25	97.7	77.0	112.7	92.0	.816	692	616	1.124	"
26	106.2	97.2	121.0	112.0	.926	510	448	1.138	"
27	18.6	3.9	33.4	18.7	.560	246	231	1.066	"
28	8.2	1.6	23.1	16.5	.714	154	147	1.046	"
29	6.8	1.1	21.5	15.8	.735	142	134	1.063	"
30	53.2	38.0	68.0	52.8	.776	447	400	1.117	"
31	20.5	14.5	35.3	29.3	.830	212	187	1.131	"
32	17.4	13.5	32.1	28.2	.879	165	148	1.116	"

TABLE NO. II.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
No. of Test.	Boiler Pressure Absolute.	Back Pressure Absolute.	Orifice Pressure Absolute.	Ratio of Absolute Back to Absolute Boiler Pressure.	Steam by Experiment.	Steam by Napier's Rule.	Coefficient.	Steam by Thermodynamic Eq.	Coefficient.
1	122.1	17.7	76.5	.145	302	308	.981	308	.981
2	122.8	18.2	77.8	.148	306	310	.987	308	.993
3	117.7	18.0	74.4	.153	291	297	.979	295	.984
4	123.5	47.7	78.1	.386	306	312	.981	311	.983
5	122.5	30.4	77.7	.248	305	309	.986	308	.992
6	123.2	34.3	78.2	.278	305	311	.981	308	.990
7	122.6	36.0	78.1	.294	304	310	.982	306	.993
8	126.1	102.2	103.5	.810	252	255	.989	262	.979
9	123.3	111.5	112.9	.904	186	187	.995	190	.980
10	123.6	112.7	113.9	.912	178	181	.986	185	.965

EXPERIMENTS WITH THE PHONOGRAPH RELATING TO THE VOWEL THEORY OF HELMHOLTZ.[1]

By CHARLES R. CROSS and GEORGE V. WENDELL.

Received February 27, 1893.

The value of the phonograph as an aid in the study of the theory of vowel tones was recognized almost immediately upon the invention of that instrument, and it was employed for this purpose as early as 1878 by Messrs. Jenkin and Ewing,[2] and also by Dr. C. J. Blake in connection with one of the present writers. The experiments of the last mentioned observers were designed to aid in the solution of the question whether it is a fact, as assumed by Helmholtz, that each vowel possesses a distinctive character due to the presence of a particular tone or tones, which are the resonance notes of the mouth cavity when shaped for the utterance of the corresponding vowel. The method employed and the results obtained were described in a letter to *Nature* (Vol. XVIII, p. 93), so that a brief reference to them will suffice.

The plan followed was to speak a vowel into the mouthpiece of the phonograph when the cylinder was revolved at a certain rate, and then to reproduce the tone with a varying rate of revolution, both faster and slower than that used when the record was impressed upon the cylinder. Any particular resonance note, if present, would then have its pitch raised or lowered, and presumably the vowel sound would be correspondingly altered.

This was found to be the case, the vowel apparently changing with change of speed of the cylinder. For example, the vowel *ō* changed to *e* on increasing the speed to a rate considerably above that at which the former vowel was impressed upon the recording cylinder, and fell to *â* when the speed of the cylinder was carried considerably below

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[1] Read at a meeting of the American Academy of Arts and Sciences, May 24, 1892.

[2] See Nature, Vol. XVII, p. 384; Trans. Roy. Soc. Edinburgh, Vol. XXVIII, p. 745.

that at which the record was made.[1] Various other results of the same character were secured.

The accuracy of these results was questioned, but they were shortly afterwards confirmed by A. G. Bell and F. Blake,[2] and others. The only form of phonograph in existence at that time, however, remarkable as it then seemed to be, was extremely crude and imperfect, the tin-foil employed as a medium for receiving the record not being well fitted to receive and retain the delicate impressions of the sounds of the human voice. Upon the commercial introduction of the modern phonograph of Edison, in which a cylinder of wax replaces the tin-foil, and which leaves little to be desired so far as clearness of articulation is concerned, it appeared to be desirable to repeat the early investigations just referred to with the improved instrument. After our work was well advanced, our attention was called to investigations in the same direction by Hermann.[3] But his very valuable researches on vowel tones, and their study by the aid of the phonograph, have mostly employed methods other than the one under consideration, and only some general results by this particular method seem to have been published by him. These are in accordance with the observations described in the letter to *Nature* already mentioned.

The method followed in the present series of experiments is identical with that used in the earlier studies referred to. As the cylinder of the modern phonograph is rotated by an electro-moter furnished with a good speed-governor, it was easy to vary the speed within moderately wide limits, and to keep it at a tolerably definite and known rate, which was done in most of our experiments. In some of these, however, only general results were sought for, and no attempt was made to measure the speed.

We proceed to give a detailed description of a few of our more general preliminary experiments, which is followed by a statement in tabular form of the later and more precise observations. In the former, we have noted only those sounds which seemed most prominent in the different series.

(1) The vowel *ö* was spoken into the speaking-tube of the phonograph when the cylinder was rotated at the lowest speed at which it

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[1] In this paper the sounds of the different vowels are denoted by the conventional signs employed in the Century Dictionary. Certain other signs used are explained later.

[2] See American Journal of Otology, Vol. I, p. 163.

[3] Cent. f. Physiologie, 1890, Vol. IV, p. 242; Pflüger's Archiv, 1890, Vol. LXXIV, p. 42.

was possible to drive it with the governor used — about one half revolution per second. The vowel was then reproduced with a gradually increasing speed of the cylinder up to the highest possible speed — about three and one half revolutions per second. The experiment was repeated several times, and with different observers. The sounds heard as reproduced were

ŏ ō ou ü a.

(2) The vowel *ŏ* was spoken into the mouthpiece at a speed of about one revolution per second. On reproducing it at lower rates down to about one half revolution per second, the vowel sound was still heard as *ŏ*, but becoming more and more guttural, until at the lowest speed it sounded like a deep gurgle.

(3) The vowel *ō* spoken with a moderately low speed of the cylinder, and reproduced with gradually increased speeds from the lowest possible to the highest, gave the following sounds:

ŏ ō ọ̄ ou ä a.

(4) The vowel *ō* was spoken into the instrument at the highest attainable speed, and repeated a number of times, the speed of the cylinder meanwhile being lowered gradually, so that the vowel in question was impressed upon the cylinder at a variety of speeds, from the highest to the lowest. It was then reproduced with gradually increasing speed of the cylinder, beginning with the lowest possible. This procedure was well adapted to give a large number of vowel sounds in succession. The reproduced vowels noted were

ŏ ō ọ̄ ou ä a i.

(5) The vowel *ō* spoken at the lowest possible rate was reproduced at gradually increasing speeds. The sounds heard were

ō ä a e i.

(6) The vowel *ō* spoken at a rate of about one revolution per second was reproduced at a lower speed as *ŏ*, and at the lowest attainable speed as a very deep guttural *ŏ.*

(7) The vowel *ē* spoken with a cylinder speed of about one revolution per second, and reproduced at speeds varying from the lowest to the highest attainable, gave the following sounds:

ü ē ẹ̄ i.

The last mentioned sound was given out only at the extreme upper limit of speed.

(8) The vowel $\bar{e}$ spoken with a speed of about two revolutions per second was reproduced at a lower speed as $\ddot{\imath}$, but at all attainable higher speeds still retained its vowel character as $\bar{e}$.

The following tables contain the results of later experiments in which the speed of revolution was measured. Table I contains results reached when a single vowel was sounded and reproduced at various speeds. The first column contains the serial number of the experiment; the second, the name of the vowel; the third, the rate of revolution of the cylinder of the phonograph when the vowel was spoken into the mouthpiece and its record impressed upon the wax; and the fourth, the vowel sounds observed when the spoken sound was reproduced at various speeds, R_1. The speed is denoted in revolutions per second.

TABLE I.

No.	Vowel Spoken.	R	Vowels Reproduced.					
1	$\bar{o}$	1	R_1	$\frac{1}{2}$	1	2	3	
			Vowel reproduced	$\ddot{o}$	$\bar{o}$	*ou*	*ou*$\ddot{\imath}$	(almost $\ddot{a}\ddot{\imath}$).
2	$\bar{o}$	1	R_1	$\frac{1}{2}$	1	2	3	
			Vowel reproduced	$\ddot{o}$	$\bar{o}$	*ou*	*ou*$\ddot{\imath}$	(less sharp than in 1).
3	$\bar{o}$	2	R_1	1	2	3	$3\frac{1}{2}$	
			Vowel reproduced	$\ddot{o}$	$\bar{o}$	$\bar{o}$	*ou*	
4	$\ddot{a}$	1	R_1	$\frac{1}{2}$	1	3		
			Vowel reproduced	δ	$\ddot{a}$	*a*		
5	$\ddot{a}$	3	R_1	3	2	1	$\frac{1}{2}$	
			Vowel reproduced	$\ddot{a}$	δ	$\bar{o}$	$\ddot{o}$	
6	$\ddot{a}$	2	R_1	$\frac{1}{2}$	1	2	3	
			Vowel reproduced	$\ddot{o}$	$\acute{o}$	$\ddot{a}$	*a*	
7	$\bar{a}$	$\frac{1}{2}$	R_1	$\frac{1}{2}$	1	2	$2\frac{1}{2}$	3
			Vowel reproduced	$\bar{a}$	$\ddot{a}$	*a*	*e*	*i*
8	*i*	1	R_1	$\frac{1}{2}$	1	2		
			Vowel reproduced	*e*	*i*	*i*		

TABLE II.

No.	Vowel Spoken.	R	Vowels Reproduced.
9	ö ō	1	R_1 . . . . . 1 2 Vowel reproduced . . ö ō ō ou
10	ō ö	1	R_1 . . . . . 1 2 Vowel reproduced . . ō ö ou ō
11	ē ē ō	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ē ē ō ē ē ou ē̇ ē̇ ä
12	ē ō ē	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ē ō ē ē ou ē ē̇ ou♯ ē̇
13	ē ö ē	1	R_1 . . . . . 1 2 3 3½ Vowel reproduced . . ē ö ē ē ō ē ē̇ a ē̇ ē̇ a♯ ē̇
14	ō ē ē	1	R_1 . . . . . 1 1½ 2 Vowel reproduced . . ō ē ē ou ē̇ ē ou♯ ē̇ ē̇
15	ō ö ä	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ō ö ä ou ō a ou♯ å e
16	ö ō ä	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ö ō ä ō ou a ō̇ ä e
17	ä ö ō	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ä ö ō a ō ou a♯ ō̇ ou♯
18	ō ē ä ī ö	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ō ē ä ī ö ou ē̇ ä♯ ī♯ ō ou♯ ē̇ ī̇ a♯ ō̇
19	ō a ī ö ē	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ō a ī ö ē ou a♯ ī̇ ō ē̇ ou♯ a♯ ī♯ ō̇ ē̇
20	ö ō ä e i	1	R_1 . . . . . 1 2 3 Vowel reproduced . . ō ō ä e i ō ou a♯ ė i̇ ō̇ ou♯ a♯ ī u♯

In addition to what has already been said as to the conventional signs used by us to indicate the different vowel sounds, it should be stated that in the preceding tables and subsequent pages a ♯ following a vowel indicates a rise in its characteristic note, generally with an accompanying nasal quality Thus in (1), (2), (12), and elsewhere, the sound denoted by *ou*♯ is nasal. In (1) it approaches a nasal *ä*. In (13), and elsewhere, the sound denoted by *a*♯ lies between *a* and *e*, and closely approaches or even passes into the French nasal *in*. The sound *ä*♯ in (18) approaches *ī*. The sound *ī*♯ in (18) is strongly nasal in quality. The *u*♯ in (20), however, is not at all nasal, but is a very high *u* with a resonance note much higher than that of *ī*.

Several series of experiments were also tried in which a number of vowel sounds in succession were impressed upon the cylinder, and sub-

sequently reproduced at different speeds — a method which enables one to compare more readily the changes in the various vowels with one another. The results of these comparisons are found in Table II.

A careful comparison of the various changes indicated in the preceding tables will show a close accordance between the results of different experiments. But there will be noticed a few apparent discrepancies. Thus in (15) $\bar{o}$ rises to *ou*♯ and in (16) to *ä;* in (15) *ö* rises to *å*, and in (16) to $\bar{o}$, while $\bar{a}$ rises to *e* in both series. These and such other like results as exist are due chiefly to the fact that it was difficult to measure the speed with any great exactness at the higher rates, or to maintain this speed absolutely uniform, and a slight increase in speed would suffice to change the results as stated in (15) to those of (16). The vowel $\bar{a}$ sustains the same change in each, notwithstanding this difference in speed, because the *e* into which it changes is more persistent than the other vowels in the series, as will be more fully explained a little later. It will also be instructive to compare (16) with (17). The $\bar{a}$ in the former rises to *e* on passing from one to three revolutions, while in the latter it rises only to *a*♯, which sound would have passed into *e* on a slight increase of speed. In (16) the speed was doubtless a trifle higher than in (17) as well as in (15).

It must also be remembered that at certain stages a sound will be on the point of passing from one recognized vowel into another, so that it may be difficult to denote its sound exactly by any of the conventional signs usually employed. For this reason it might be preferable to substitute the symbols used by Mr. Melville Bell in his "Visible Speech."

Furthermore, we have noticed in some cases that a very trifling difference in the quality of the vowel impressed upon the cylinder at a low speed may cause a decided change in the vowel sound given out at a considerably higher speed. Thus, the vowel *e* reproduced at increased speeds passes into a high $\bar{\imath}$ and then into *i*. But if a series of *e*'s be spoken into the instrument, it is possible to find a speed of reproduction such that some of them are heard as $\bar{\imath}$'s and some as *i*'s. On raising the speed the $\bar{\imath}$'s tend to rise to *i*, and on lowering it the *i*'s tend to fall to $\bar{\imath}$.

Some interesting peculiarities of different vowels were observed in the course of our experiments.

It was noticed that, in several cases where the vowel seemed to have quite lost its peculiar quality, there was nevertheless a certain

reminiscence, so to speak, of its original character remaining. This appeared to be due to the logographic differences among the various vowels. Of course, the logographic characteristics of any vowel are not altered by the speed at which the sound is reproduced, so that in many cases we necessarily reproduce a sound with a characteristic resonance note belonging to one vowel and a logographic character belonging to another. Where the logographic character is clearly marked, this may enable one to recognize the vowel sound originally spoken into the phonograph, even though the characteristic resonance note is quite changed. This is especially true when the vowel forms part of a word.

There also appears to be a great difference among different vowels in what we may call the persistence of their vowel character when the speed of reproduction is varied. Whereas some, as *ö*, *ō*, and *ä*, change their character completely on increasing the speed, say to double its original rate, others change far less. The vowel *ē* seems particularly persistent under a large change in speed. With it as with all vowels there is necessarily a shortening at higher speeds, as denoted in the tables by a dot placed under the letter; but apart from this the speed may be varied more than for any other vowel studied by us before any marked change in its quality appears.

In order to form some estimate of the extent to which the context would influence the judgment as to the character of a vowel sound, the experiment was tried of reproducing at different speeds a sentence or verse spoken into the instrument. Thus the words from a negro melody containing the vowel *ō* many times repeated were spoken, as follows:

"Roll, Jordan, roll!
Roll, Jordan, roll!
I want to go to heaven when I die
To hear old Jordan roll."

These words were impressed upon the cylinder at a speed of three revolutions per second, and were reproduced at speeds of two revolutions and one revolution per second respectively, with the following results. At two revolutions the *ō*'s were in all cases plainly recognizable though lengthened; but at one revolution the quality was completely altered, *ō* having become changed to *ö* in all cases. Like results were observed in other sentences containing the vowel *ō*.

In like manner the sentence "This was the noblest Roman of them all" was impressed upon the cylinder at a speed of one revolution. Reproduced at two revolutions, the *ō*'s had a sound which closely ap-

proached *u*. Lowering the speed slightly changed it to a very short *ŏ*; and raising the speed slightly, to a clear *u*. At three revolutions the quality of all the vowels was so completely changed that the sentence was entirely unintelligible even to one knowing what it really was, the rapidity of the speech, as reproduced, however, contributing largely to this lack of intelligibility. To avoid this latter source of difficulty, the same sentence was spoken into the instrument at a speed of two revolutions, and reproduced at one revolution and also at three revolutions. At the lower speed the *i* in "this" changed to *a*, so that, the *s* in "this" being rather indistinct, the word seemed almost to have changed to "that." But the *ŏ*'s, while deepened in quality, did not apparently actually change to *ö*. Lowering the speed still more caused a closer approach to this, but the *ŏ* quality still seemed to persist. This was doubtless due in part to the association of the sound with the word, and in part to the acoustic effects due to logographic pressure, and to the connection of the vowel with the preceding consonants. To study these, the same sentence, preceded by four *ŏ*'s and followed by the same number, was spoken at two revolutions and reproduced at one revolution. The separate *ŏ*'s seemed to fall to *ö*, and there was likewise a perceptible fall in the same sounds in the body of the sentence, but these seemed still to retain their *ŏ* sound. To test the matter still further, the same sentence with the *ŏ*'s mispronounced was spoken into the phonograph, at a speed of one revolution, as follows: "This was the nöblest Röman of them all." On reproducing it at two revolutions the *ö*'s changed to *ŏ*, and the sentence was clearly heard with the mispronounced words rectified, and as distinct as if they had been properly pronounced when impressed upon the cylinder, and reproduced with the same speed of the cylinder as when uttered. Lowering the speed below one revolution, the *ö* sounds became still deeper and clearer.

As the vowels *ŏ* and *ö* are more persistent in their character than some others, a similar experiment to those already described was tried with the line "Though the harbor bar be moaning." This was impressed upon the cylinder at a speed of one revolution per second. Reproduced at two revolutions, the words "härbor bär" became "harbor bar" (*a* as in *hat*), and at three revolutions the *ă*'s in the same words assumed a nasal quality somewhat approaching the French nasal *in*.

The further experiment was performed of sounding the five vowels *ā ē ī ō ū* successively into the cylinder when this made one revolution

per second, and reproducing them at three revolutions. It was found that the quality of the different vowels was altered so that they were unrecognizable by one ignorant of the sounds which had actually been spoken by the voice. The same result was reached with the vowels *ä e ē ō ö*, and also with *ö ō ä e i*. The last mentioned series, which is No. 20 in the tables, was impressed upon the cylinder at a speed of one revolution, and reproduced at two and at three revolutions, approximately. At two revolutions the series seemed to have changed to *ō ou a♯ e i*, the *a♯* closely approaching the French nasal *in*. At three revolutions the sounds heard were *ō ou♯ a♯ ī u♯*, the *a♯* being a clear nasal *in* and the *u♯* a very short *u* with a high resonance note.

The limitations in speed of the phonograph cylinder, as the instrument is constructed for practical purposes, have thus far prevented us from carrying the range of changes in the pitch of the reproduced sounds as far as is desirable. This defect we purpose to remedy by suitable modifications in the driving gear of the apparatus. For reasons already explained, it is also desirable to measure the relative rates of rotation more accurately than we have been able to do with the commercial form of the phonograph.

We hope that we may be able to continue this investigation by a more systematic study of the behavior of the reproduced vowel sounds, and likewise to consider the effect of changed pitch in reproduction upon the various consonantal sounds.

In connection with the present subject, it is interesting to consider the unconscious testimony to the existence of different characteristic resonance notes for the different vowels which is given by various onomatopoetic words. The words used to denote various sounds form an excellent example, as will appear from the following list of a few such words in which the pitch of the sound denoted is higher as the list proceeds: boom, gurgle, roll, toll, roar, slump, thump, crash, smash, crack, snap, bang, jingle, ring, hiss. It will be observed that the vowels in the later words are those with higher resonance notes.

ROGERS LABORATORY OF PHYSICS,
May, 1892.

THE STUDY OF ARCHITECTURE.

By FRANCIS W. CHANDLER, Professor of Architecture.

Received March 4, 1893.

Vitruvius perceived clearly the many-sidedness of architecture when he enumerated the varied qualifications which he considered necessary for the finished architect. To reach his high standard to-day, even if nothing more was required, would tax the powers of the best of us to the utmost. But could he return and see the progress of civilization during the centuries since he wrote his first specification, and note the numberless additional burdens that have been placed on the architect's shoulders in consequence, it would surely seem to him well-nigh impossible to find any single person capable of undertaking so much. In this generation alone we have seen whole trades come into existence — trades which at the beginning simply catered to the luxury of the few rich, but have now become a necessity to the whole people, as, for instance, those which have to do with sanitary science, electrical engineering, heating, and ventilation; while others, as iron construction and decoration, have so expanded as practically to cover new fields. These add much to the comfort and attractiveness of our lives, but they also add immensely to the architect's labors, which even eighteen hundred years ago Vitruvius considered so exacting.

Glancing over the field of necessary operations for any architectural work, every one must be impressed, and most of all the architect, with the responsibility called for in this difficult profession.

So before beginning its study one should ask himself if he has the primary qualifications for success, and in what order they should be considered. The student must remember that he has no clear field before him. Competition is steadily increasing, for no profession offers greater attractions to the cultured scientific mind.

First of all he must be strongly attracted to this study. He must then bring industry to bear upon his work, and if he has common sense, which is the one universally appreciated element, he should feel

his chances of success to be fair. If after this he develops genius, he may be thankful.

Professional training may be divided into two parts — the technical and the artistic; in the former, science and mathematics are the principal studies; in the latter, drawing, designing, and study of historical precedent. In regard to general culture, its value is too self-evident to need a special appeal. It is impossible for the architect to have too broad a culture. He must be able to enter into the interests of his cultivated client, with whom no profession can have more intimate relations. Any work in the Fine Arts will be better done by a man who has a broad horizon and generous views of life.

The study of scientific construction is a necessary part of an architect's training. Construction and design are inseparable, and must be studied together. All experience in the architectural practice of to-day shows that the architect should not only be a skilled artist, but also be so scientifically trained as at least to appreciate all constructive questions. In the great office buildings, built for investment, there is no more important factor to remember than that only enough material shall be used to insure their stability, or else capital is wasted. Science tells us the nature of materials, and materials must govern design. For example, engineering practically discovered the use of iron as a building material; science has shown us the exact conditions for its use; and this knowledge of using fitting material and designing in accordance with it is a fundamental principle of architecture. A man who does not give due weight to construction cannot design, because in true architecture the artistic design simply reënforces and makes intelligent the construction. Neither can a great architectural work be thoroughly appreciated except by one who has been so scientifically trained as to understand the difficulties overcome, as well as the skill with which the design tells the story.

The study of history is all important. One must understand and follow the growth of a great style, from its beginning to its culmination and to its final merging into the next epoch. For the history of architecture is like a series of waves, the crests the perfected styles, and the hollows the transition from the one into the next following. In this study of architectural history one great fact stands forth — that these monuments have character and appeal to us strongly because they are the results of the effort of whole ages, rather than of any individual. There was no question with the old masters what

style was best adapted for the immediate work in hand; there was only the one style, and because they had grown up in its traditions they worked in it so naturally that their work could not be otherwise than good. In the old time originality confined itself to adapting good work, when seen, to the objects in hand, preserving the life and spirit of the original, which were its supreme attractions.

Every great style tells the same story. Even the Gothic, so commonly thought to have been called into life because connected with the religious awakening of the Middle Ages, owes its splendid results rather to the fact that this great movement took place at that time. The style was not used solely for religious purposes. There was no difference in the detail of molding or carving used for cathedral or other building — the same grotesque forms were to be found in the one as in the other. The difference was simply in degree of richness, there being more money for the cathedral than for any other building. When the Renaissance came, there was the opening up of universal knowledge and intercourse. Until then all architectural influences were practically local. Now the whole world is open to us to choose our model from, which before could not be farther away than in the next village.

Here is the modern problem — a big world with its treasure at our command. The same processes for design have to be gone through with now as in earlier times. We can found our work on Gothic or Renaissance. We can be eclectic; there is no law against it, but probably the work of any individual architect will be better if it is confined to a limited field of study. I recommend you as students to the most careful, solid, and persevering study of the Classic and Renaissance work. Because in the Orders culminated all architecture before them, and in the Orders was the birth of the great styles that came after. Because there is no more interesting period of history, and especially architectural, than that of the Italian Renaissance. Because there is no phase of the world's architecture of more interest to us as a nation, since it is so readily adapted to our needs. Because it requires learning, historical knowledge, and great accuracy. It involves all the fundamental principles of design, and there is no concealing of errors as in the more picturesque styles.

After you leave school you must meet the world in your own way, but if you have had a solid training in balance, proportion, light and shade, scale, color, etc., in a style requiring the nicest study of all of

these, you can be trusted to become eclectic or to work in any style you may ally yourself with.

The value of a good library cannot be overestimated, and the opportunity to use one should never be neglected. The use of such libraries as are a part of our principal architectural schools is an education in itself; there can be no end to the profitable labor that may be expended there. The student's sketch-book must be ever at hand. He will be adding constantly to his stock of architectural precedent for future use, as well as to his ability in drawing, and let him remember that designing is a natural sequence of good drawing, and the greater his facility in this the less willing he will be to let a design go forth until he is satisfied that it is his best.

Besides his drawing, he must learn to analyze for himself; he must not be satisfied with the mere fact that a building pleases him, or the reverse, he must find out why it is so, and be able to express the reasons. This is the true spirit of criticism. He should ask himself: Is it the harmonious proportion, or effective details, or the nice balance of light and shade? etc., etc. This habit of analysis is of the greatest importance, and the student will find he is storing his mind with certain rules which will be of great service in his future work, and all the more valuable because they have been thought out by himself and not accepted secondhand. Do your own thinking; study the old work; see in your own mind how you would adapt it to the present time, preserving its old spirit.

There is one other important study too much neglected — that of the arts of Painting and Sculpture in their relation to Architecture. Every good style has inevitably called to its aid the sister arts, and every architect should so understand them that when present in his work they should be an integral part of his design.

It is often said that it is an open question whether Michelangelo or Raphael was greater as painter, architect, or sculptor, and that it is hopeless to aspire to do as good work as theirs; but as we study our history and these artists' work and think for ourselves, we shall easily see that they were not equally good in all, that architecture was much the least part of their strength. In fact, Mr. Symonds speaks of Michelangelo as continually protesting that architecture was not his trade.

We shall see that such men as Brunelleschi, Alberti, or Bramante, who did not aspire to be great painters or sculptors, were greater in

architecture than Michelangelo or Raphael, but at the same time they were so at home in painting and sculpture that they knew how to heighten the effect of their work by the aid of these arts, and knew exactly what such painting or sculpture should be. It was because painters and sculptors in those days were called into being by architecture that they understood the art of the architect so well, that there was perfect harmony among the three. And it is from this point of view that we must get back to the old way of doing work. Until architect, painter, and sculptor are intimately associated again, studying each other's art, advising one another, working together for the good of all instead of the individual, we cannot have perfect architecture.

> One science only will one genius fit:
> So vast is art, so narrow human wit.

So while we only attempt to be expert in our own art, let us understand so well the language of the others as to make it a part of our own. This close alliance of the arts is seen in all the great styles, from the earliest times, in the painting and hieroglyphics of the Egyptian walls and columns, the friezes of the Greek temples, the mosaics of the Byzantine churches, the frescoes of the Italian Gothic, and the noble portals and stained glass windows of the Western and Northern mediæval Gothic. This close alliance is shown better, perhaps, in Gothic architecture than in any other style. In the South, the heat and brilliant sunshine called for cool and shady retreat; windows were reduced to the smallest size, and the great wall spaces called for color, and on these the painters exercised their art; they were the only fields for their talent until the Renaissance. Stained glass was to the Western and Northern Gothic what fresco was to the Southern. The gloomier, colder climate called for more sun to be let into their buildings, and the great windows were at once seized upon as the proper field for decoration, and they were made larger and larger until there was no wall space left. In all these instances the design included the three arts, and not one could be taken away without great injury to the whole.

I write this so that the young architect may see how much there is for him to do to help create a school to make architecture a living art as it was in the olden time. He must have the highest ideal and aim to excel in his own art. He must form a close broth-

erhood with the painter and sculptor. He must always remember that his education is never complete. He must not think because he has ended his course at school he is qualified to practice architecture. His degree can only prove that he is solidly prepared in the fundamental principles of his profession; that he can stand on his own feet, and knows in what direction to walk. He must next have the training which is found in the office of a busy architect, under experienced men, on actual work, and with skilled mechanics. The lessons thus learned are invaluable and never forgotten. This is an experience which must be had to give one the right confidence in himself and to obtain the confidence of the public.

And until this training has been thorough and prolonged our friend should resist every inclination to join that body of young architects whose work may be fairly criticised by the two words, "insufficient training." So when the time does come for him to call himself a professional architect, if he has a high ideal and is strong enough to live up to it, he will lead the public, and not be led by it.

THE ACTION OF COMPOUND DYNAMOS WHEN RUN IN PARALLEL.

By WILLIAM L. PUFFER, S.B.

Read March 23, 1893.

WHILE arranging some details of the wiring plan for connecting up two compounded railroad generators so that they could be tested for commercial efficiency, while under the conditions of full load, by one of the purely electrical methods my attention was drawn to the fact that the usual method of adjusting such dynamos so that they would give a constant potential at their terminals irrespective of the current flowing could not be a correct one unless the machines were exactly alike in all respects.

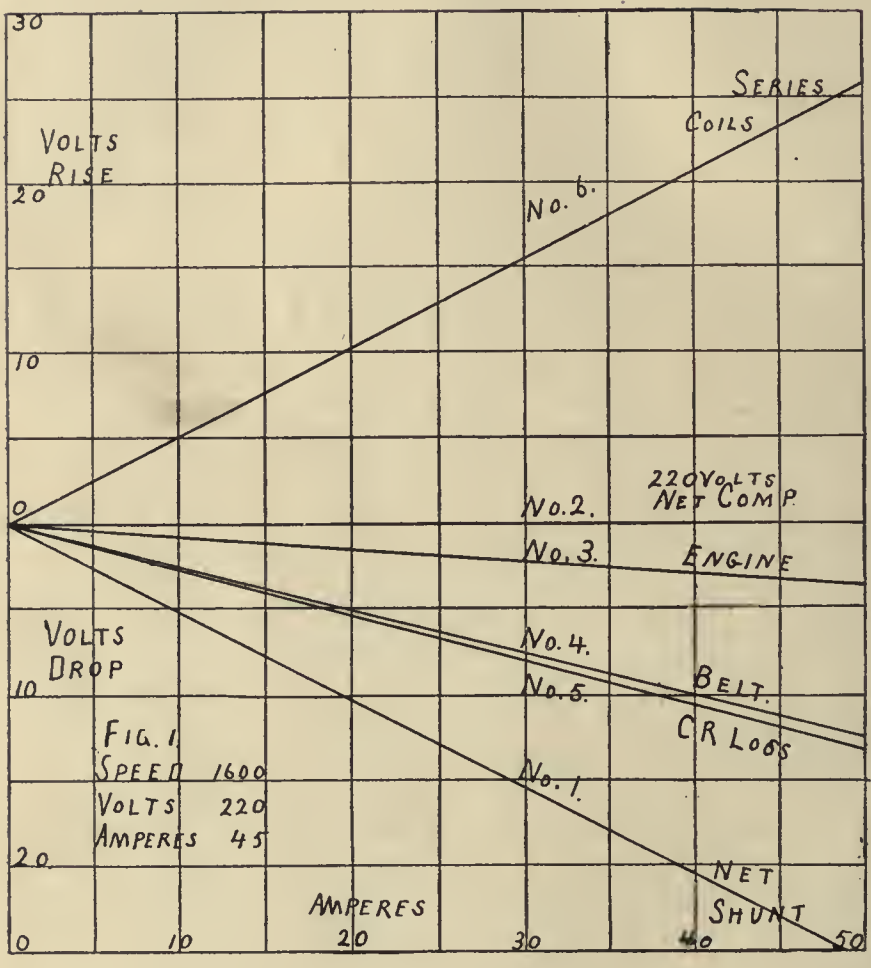
The general practice is to over-compound such generators, and then to experimentally shunt the series coils with a German silver ribbon until the desired "0 per cent. rise" is obtained; this is well enough for a single dynamo, but when two or more are in parallel, with their series coils also in parallel, the German silver shunts do not in any way aid the different dynamos in equalizing their load, and any original structural difference will cause the load to divide unequally. I found that in all of the power plants that I could then visit (winter of 1890–91) there was very decided evidence of this tendency to unequal division of the load, and it could readily be seen that some machines were always taking more than their share of an increase of load and tending to do too little when the load fell off. There seemed to be a very common knowledge of the trouble in making the dynamos work well together, but there seemed to be no attempt to help the matter, or, as far as I could find, any knowledge of where the fault was.

A year ago the Thomson-Houston Electric Company was so kind as to loan the Institute a pair of similar over-compounded motor-type dynamos which had been adjusted for "0 per cent. rise;" these dynamos were similar to the power generators of that company, and enabled

me to make a study of the action of compound generators connected in parallel. The dynamos were belt driven from the main shaft in the dynamo room of the Physical Department and were under good average running conditions. A short piece of shafting was arranged so that by slipping the pulleys slightly the two armature shafts could be mechanically connected, and therefore must at all times be at the same speed. Suitable water resistances connected to each machine were used for varying the output of the armatures. Double-pole jackknife switches of the type generally used were provided for connecting the two dynamos in parallel. A dead beat ammeter was placed in each armature circuit, and an Ayrton & Perry spring ammeter in the equalizing wire; a Weston voltmeter was used in connection with pressure wires to measure the voltage of either armature.

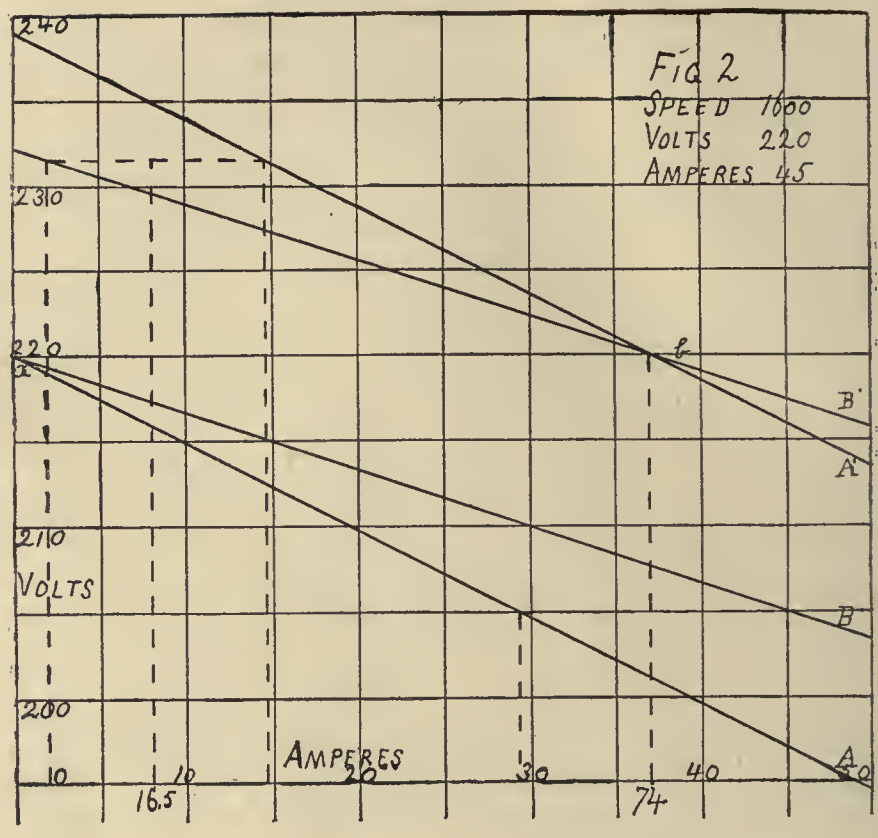
The object of this paper is not to describe these tests, but rather to explain a method of investigating the action of compound dynamos in parallel, and to point out the cause of some of the difficulties commonly met with as well as to show that it is comparatively a simple matter to avoid them; so that the results of these tests, although very interesting, will not be given except as explanatory examples.

Considering first the action of a single shunt dynamo driven by a belt from an engine doing no other work, if now the load be gradually increased to the maximum amount that can be safely carried, and if from time to time simultaneous values of the current and voltage be taken and plotted as in Figure 1, Curve 1 — for want of a better name let us call this the *net* characteristic curve of the combination — this curve would be a horizontal line (Curve 2) if it were not for the sum of a number of actions all of which tend to lower the electromotive force; beginning at the engine there is the usual fall of speed as the load increases, due simply to the action of the governor (Curve 3), the dynamo pulley lags behind more and more on account of the increasing slip of the belt as the transmitted power becomes greater, these two changes are plotted in Curve 4; in the dynamo armature there are the usual demagnetizing reactions which lower the electromotive force; and finally there is the fall of potential in the armature wires as well as all the necessary leads, fuses, ammeters, and connections up to the distribution point or bus bar where the voltmeter was connected (Curve 5). These separate losses may be plotted separately, and their sum will equal the amount that the net characteristic will fall below a horizontal line.


30
20
10
0
VOLTS RISE
VOLTS DROP
10
20
SERIES COILS
No. 6.
220 VOLTS NET COMP
No. 2.
No. 3.
ENGINE
No. 4.
No. 5.
BELT.
C R LOSS
No. 1.
NET SHUNT
FIG. 1
SPEED 1600
VOLTS 220
AMPERES 45
AMPERES
0
10
20
30
40
50

Inasmuch as the net characteristic in this combination is a very drooping one, it would be necessary to use hand or automatic means of adjusting the voltage as the current changes so as to keep the mains at the normal value — the effect of this regulation can be considered to be a raising or lowering of the net characteristic bodily without any material change in the inclination. Such regulation is always too late by at least the time necessary to produce a change of the indicating device, yet if this amount is small and the demand for current is not very variable such a combination is perfectly satisfactory, and even desirable, on account of the sudden fall of voltage on a short circuit which lessens very materially the shock to all parts of the system.

A second similar combination can be connected in parallel to this first one and the load divided between them — all that is necessary is to have the voltage of the incoming system very nearly equal to that of the bus bar or common point and then to close the switches. Evidently if the voltage of the new dynamo were just right no current could pass in either way, but if it were somewhat too high then a part of the total current would change from the old dynamo to the new one. If, for example, the new dynamo were x volts higher than the old, then the current through it would be of such value as would cause a fall of $\frac{1}{2}x$ on the net characteristic, and the current from the old dynamo would be smaller by the same amount. When the net characteristic is quite drooping, as is always the case with shunt dynamos, there is little or no danger of even overloading an armature when connecting it to the mains, in fact it is common practice in central stations to judge of the voltage of the new dynamo by the looks of its pilot lamp. If the two units (dynamo with its engine) have similar net characteristics, any change of load, sudden or not, will divide equally between them; but if, as usual, the characteristics are not similar this will not be the case, and we shall find that the unit with the steepest characteristic will not take its share of the work if the load increases, and will take more than its share with a decrease of load.



Let the lines A and B in Figure 2 represent the net characteristics of the two units; when the armatures are in parallel and delivering no current, let the voltage be brought to 220 volts, then the two lines are common, as at a. A sudden current of 74 ampères will cause the voltage to drop to 205, and B will deliver 45 and A 29 ampères. If now the load is equalized with 37 ampères in each armature, and the voltage brought up to 220 by the use of the regulating rheostats, the lines A and B are raised to A^1 and B^1 respectively, being common at b; now let the total current be diminished from 74 ampères to 16.5 ampères, a similar construction shows that the voltage will rise to 231.6 volts, and that A will deliver 14.5 ampères, and B 2 ampères.

When two dynamos with net characteristics like these are subject to a rapidly changing load, it is impossible to evenly divide the

current, and a balance has to be struck between the limits on the one hand of the dynamo A driving B as a motor — which will, of course, do no especial harm other than to waste energy in heating and sparking — and on the other hand of melting the fuses of dynamo B during an unusually heavy call for power, which might have been safely carried by the fuses if equally divided. It is plain that in this case the combination of the two dynamos is not double the capacity of either one, and the commercial efficiency is considerably lessened by the want of similarity of the net characteristics.

It is very desirable on many accounts to have a dynamo which will automatically maintain an approximately constant potential at the switch board irrespective of the amount of current flowing. We may add a series winding to the dynamo of just sufficient number of turns to give such a magnetizing effect as will produce a voltage which shall be at all times exactly equal and opposite, to the combined effects of the change in engine speed, belt slip, armature reactions, and the drop of voltage due to the resistance of the armatures, lead wires, ammeter, safety devices, and the like, up to the bus bar (Figure 1, Curve 6). The net characteristics will now be made up of the sum of the individual effects, as shown in Figure 1, Curve 2, and the voltage at the switch board would be constant at all times were it not for the hysteresis of the iron in the magnets, which will very slightly prevent the instantaneous rising and falling of the effect of the series coils, and any change in resistance due to temperature change. The temperature change will be slow, and is corrected by an occasional change of the rheostat in the shunt coil circuit and can be neglected for the present.

Should it be necessary to use a second dynamo of exactly similar design, we cannot proceed as in the case of simple shunt dynamos, for this reason: if the new dynamo is at exactly the same voltage as the mains at the switch board when connected in parallel, the combination will be unstable because if for any cause the voltage of either dynamo varies a little, so that the new machine becomes ever so slightly a motor, the effect of the reverse current in its series coil will be to weaken still more the electro-motive force, which will allow an increasing current to flow until something burns out.

If, however, there is a third connection made so that the brushes as well as the terminals of the two dynamos are connected, it becomes impossible for this reversal to happen, and the machines will run per-

fectly under all changes of load. There are in use two ways of arranging the switches (Figure 4 and Figure 5), either of which is satisfactory; but the one shown in Figure 5 is better, as the idle machine is completely disconnected from the circuits.

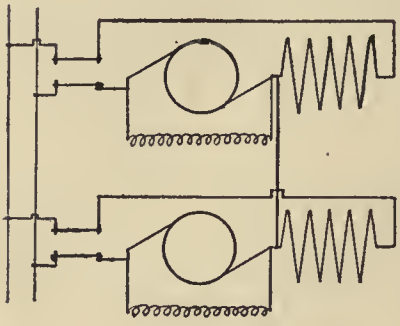


Fig. 4.

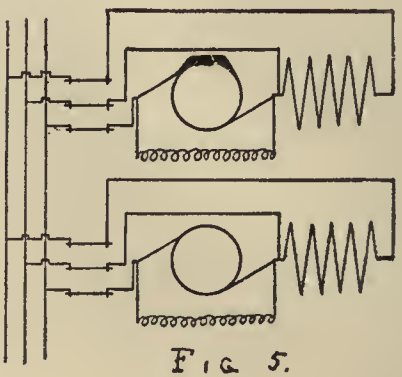


Fig. 5.

An examination of the components of the net characteristics (Figure 1, Curve 2) shows us that if the incoming dynamo be brought up to the voltage of the switch board and the switch closed, the load will instantly divide between the two dynamos with no disturbance or shock of any kind. Suppose, for instance, that the loaded machine is giving out 40 ampères, we see from the net shunt characteristic (Figure 1, Curve 1) that there is a drop of 20.5 volts, which is made up by the effect of the series coil (Curve 6), giving a terminal potential of 220 volts. When the new machine at 220 volts is switched in parallel we may first consider that one half the total current of 40 ampères passes through each of the parallel series coils, and from Curve 6 we see that the volts-rise is cut down from 20.5 to 10.2 in the first dynamo and raised from zero to 10.2 in the new one; from Curve 1 we see that for each dynamo this change of 10.2 volts just compensates for a current of 20 ampères through the armature circuits, and that the total load of 40 ampères at 220 volts will be equally divided between the dynamos.

The action of compound constant potential dynamos in parallel is generally misunderstood, and the equalizing connection is credited with a remarkable power of control over the output of such dynamos which it does not and cannot have, as can be seen by watching the dynamos

while at work as well as by examination of the makeup of the net characteristic (Figure 1). The common idea is that the equalizer has the property of compelling a lagging dynamo to take its share of the work; and we hear of the wonderful power of control which it has even over dynamos of different types and sizes running under somewhat different conditions. Now the real effect of the equalizer is to put the series coils in parallel, so that the total flow of current from the plant will divide between them inversely as their resistance without any regard as to whether this current comes from one armature alone or is divided among many, and hence it follows that it cannot in any way exercise even the slightest regulating effect on any one dynamo that it does not have over all the rest. The armatures themselves are running in parallel under the usual conditions of ordinary shunt dynamos, and the total current will divide between them under the same laws that would hold if there were no series coils at all and the current were taken from the two main wires to which the brushes are connected, as has been already pointed out in a previous part of this article.

The net characteristic of the plant is then a falling one as far as the armatures are concerned, to which has been added the automatic regulation of the parallel series coils which act together in proportion to the total flow of current from the station; and if these series coils are all alike in effect, each dynamo will have its voltage altered by an equal amount in proportion to the output of the station, but each armature will not do its share of the work unless all the net falling characteristics are exactly similar.

In a plant of such dynamos each driven by its own engine it is most likely that the net falling characteristics of the armatures will not all have the same steepness, and consequently a fluctuating load will not be shared equally by the dynamos; but it is possible to so arrange things that the series coils do not all act equally on the armatures, and if the resistances of the series coils are slightly changed by the addition of a little German silver or copper ribbon so that the total flow of current divides between the different series coils in such a proportion that the individual net armature characteristics are raised by just the right amount, we again have a plant in which a fluctuating load will divide equally among the different dynamos. The same effect may be produced by the addition of a small resistance in the armature circuit of any dynamo which takes more than its share of an increase

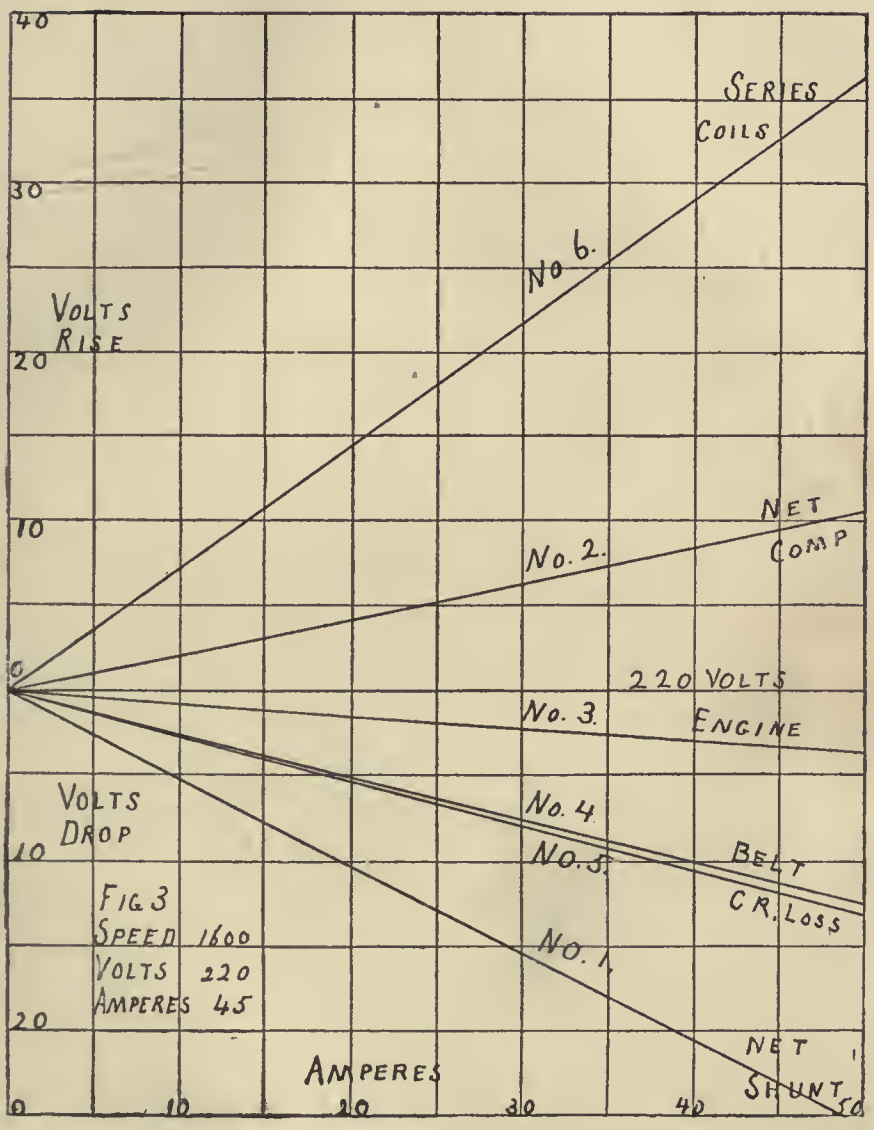
of load. A number of machines so adjusted will run together perfectly, and there will be no change of voltage or any difficulty experienced in cutting in or out by the use of the ordinary two or three-pole switch; the only precaution to be observed is to have the voltage of a new machine nearly that of the bus bars, for then, when the series coil is put in parallel with all other series coils, it will be traversed by such a part of the total flow of current as will be sufficient to raise the voltage of the armature enough to enable it to deliver its share of the total output.

If a dynamo is to be cut out it will not do to reduce its load first by the use of the resistance in the field circuit, because by this process its load is transferred to the other dynamo armatures and the station voltage will consequently fall somewhat, because the series coils are not proportionately increased. This fall of voltage may or may not be permissible, and is avoided if the switch is opened while the dynamo has its share of the load.

Sometimes it is desirable to have a dynamo which will maintain a constant potential at a point some distance from the switch board; in this case the series coils will have a few more turns, so as to make up for the fall of potential in the mains; and in Figure 3 will be found the component curves for a dynamo adjusted for approximately "5 per cent. rise." Curve 1 may be called the "net shunt" falling characteristic made up of the sum of the effects due to the engine change, belt slip, and the fall of potential in the armature circuits represented respectively by the Curves 3, 4, and 5. Curve 6 shows the rise of voltage due to the current in the series coils, and is greater than the total drop Curve 1 by an amount plotted as Curve 2, which is the net characteristic of the over-compounded dynamo.

Should the demand for current exceed the capacity of one dynamo, the addition of a duplicate machine will render it impossible to maintain a constant potential at the distant center unless both dynamos are run all the time; for while 45 ampères on one dynamo will cause a net rise at the switch board of 9.5 volts, the same total current furnished by the pair will only produce a rise of 4.75 volts.

If this want of exact regulation does not require the constant running of two dynamos at times of light load, we meet greater troubles in the process of adding the second unit when the load is too heavy for the first one. From the data given in Figure 3 it is possible to see what will happen under any given conditions when the machines


40
30
20
10
0
10
20
VOLTS RISE
VOLTS DROP
SERIES COILS
No 6.
NET COMP
No. 2.
220 VOLTS
No. 3.
ENGINE
No. 4
No. 5.
BELT CR. LOSS
No. 1.
NET SHUNT
FIG 3
SPEED 1600
VOLTS 220
AMPERES 45
0
10
20
30
40
50
AMPERES

are put in parallel. I have experimentally verified this method of predicting the actions of compound dynamos, and find it exact enough for all practical purposes.

Suppose one dynamo is loaded, what will be the effect if we connect a new one when its voltage is lower, the same as, or higher than the switch board? I will take from my notebook the results of some experiments on two dynamos with characteristic curves like Figure 3, first giving the observed results and then calculating them from the curves.

The armatures were mechanically coupled and therefore at same speed. Voltage of each was 222; 30 ampères were taken from No. 2 dynamo and voltage rose to 228, while that of No. 1 fell to 212; then by a double-pole switch No. 1 was put in parallel. Momentarily No. 2 tended to overload and drive No. 1 as a motor, but in a few seconds the load became steady at a voltage of 222, No. 1 furnishing 18 and No. 2, 12 ampères. Now by calculation from curves of Figure 3:

No. 1.	No. 2.	
222.	222.	Volts at switch board with no load.
— 7.5	— 7.5	Curve 4. Drop due to speed change when a current of 30 ampères was taken from No. 2.
214.5	214.5	
	— 8.	Curve 5. Armature drop with 30 ampères.
	206.5	
	+ 21.5	Curve 6. Rise from 30 ampères in series coils.
	228.0	Switch board voltage with 30 ampères output.

Put in parallel with same load, so no change is to be made for speed change. From Curve 6 we see that the change of half of the current from the series coil of No. 2 to No. 1 will raise No. 1.

No. 1.	No. 2.	
214.5	228.0	No. 1 with no load and 30 ampères on No. 2 and then put in parallel.
+ 12.0	— 12.0	Curve 6. Change in voltage with change of half current from series coils of No. 2 to series coils of No. 1.
226.5	216.0	
— 4.	+ 4.	Curve 5. Suppose each armature carried one half the current then there would be a drop in No. 1 and a rise in No. 2.
222.5	220.0	

We must now add a current to the highest voltage dynamo, or No. 1, that will cause a drop of one half of 222.5 — 220 volts, which from Curve 5 will be 4.7 ampères.

Then the voltage of the combination will be 221.2; and dynamo 1 will deliver 15 + 4.7 = 19.7 ampères, and dynamo 2, 15 — 4.7 = 10.3 ampères.

Calculated 19.7 ampères, dynamo 1.	10.3 ampères, dynamo 2.	Voltage of 221.2.
Measured 18 ampères, dynamo 1.	12 ampères, dynamo 2.	Voltage of 222.

Take, for another example, the case when one of these dynamos was at full load and the new machine was brought up to exact voltage and then put in parallel, and applying these methods we shall see that it may happen that all current may be cut off by the melting of a fuse.

Dynamo No. 1 at 220 volts and no load, then as the load of 45 ampères comes on, the switch board voltage will be about 230 volts.

220	
— 11.2	Drop due to load. Curve 4.
208.8	
— 11.7	Drop in armature. Curve 5.
197.1	
+ 32.5	Rise from series coil. Curve 6.
229.6	Volts at switch board.

Here the switch is closed.

No. 1.		No. 2.	
229.6		229.6	
— 16.2	22½ ampères in series coil. Curve 6	+ 16.2	
213.4		245.8	Armature voltage.
+ 11.7	Curve 5, to find internal voltage.		
225.1	Armature voltage.		

245.8	Armature voltage No. 2.
225.1	Armature voltage No. 1.
20.7	Difference of armature voltages.
11.7	Drop in No. 2 with 45 ampères.
2) 9.0	Acting to send reverse current through No. 1.
4.5	Curve 5 corresponds to current of about 17 ampères.

245.8	Armature voltage No. 2.
— 17.0	Drop with 62 ampères. Curve 5.
228.8	Switch board voltage.

Dynamo No. 1, which was loaded, has a reverse current of 17 ampères and is driven as a motor, while the new dynamo carries the load and is 17 ampères beyond its rated capacity. We see that the switch board voltage practically does not change under these conditions, but to offset this there is the sudden change of a loaded dynamo

into a partially loaded motor and an overload on the incoming dynamo. Such changes would be accompanied by much sparking at the brushes and the liability of thrown belts or blown fuses. Then we may say that if the new dynamo is of lower voltage when connected the voltage will change considerably, but there will be no shock to the dynamos; if the new dynamo is at the same voltage then the voltage will remain about constant and the armatures will receive a shock; if the new machine is still higher, then the shock will be very severe and the voltage will rise. I had no time for taking readings on the last experiment, but I did notice that the needle of a 150-ampère instrument was out of sight before a switch could be opened.

There is a very safe and simple method of throwing in a new dynamo, which does not, however, prevent a change in the switch board voltage unless a special switch is used, as shown in Figure 6, con-

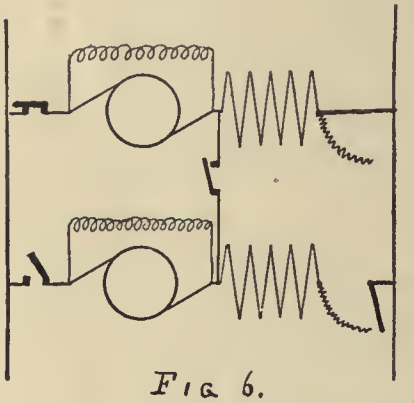


FIG. 6.

structed in such a way that the equalizing connection is first made and then the outside end of the series coil gradually connected by a switch with a few resistance points in it, while at the same time the magnetizing effect of the shunt coil of the loaded dynamo is increased to make up for the loss in the series coil. In this way the series coils can be put in parallel with no jump in the voltage. If, however, there are many similar dynamos in the plant, it might be that the change produced by throwing in a series coil without its armature would not produce a greater change than would be allowable; in such a case the safest way is to connect first the series coils, then rapidly adjust the armature voltage by the voltmeter, or, if in a great hurry, by the looks

of the pilot lamps, and then close the armature switch, well knowing that the steep net falling characteristic of the armature circuit will prevent an overload.

The common three-pole switch generally used can easily be changed so as to admit of working in this way by cutting through the top of the insulating bushing and putting a separate handle on the blade which closes the armature lead to the bus wire; then pulling the ordinary handle out will open all blades at once, but pushing it in will only connect the series coils, leaving the armature circuit open. If the wiring is so that the series coils are permanently connected and a new dynamo is thrown in by a single-pole switch, we have here a very wide range of voltages for the new dynamo, and pilot lamps may be used to judge the voltage. We have two bus bars with all the series coils joined between them; evidently, then, the total current flowing from bar to bar will simply divide between these coils inversely as their respective resistances, and we shall, in consequence, have the voltage of the armature bus bars rising and falling with the total current from the plant; and if the machines are similar all of the armature voltages will rise and fall together. If this is so, and it cannot be otherwise, these armatures are to be considered simply as net falling characteristic armatures working in parallel between the equalizing bar and the third bus bar, and the division of the load between them will take place exactly as has been previously shown for common shunt dynamos, and therefore, inasmuch as we have a very decided net falling characteristic, there is but little danger of an overload on a new dynamo armature.

It is now clear that when we wish such dynamos to run together we must first adjust the resistances of all the series coils and connections, so that when a current is passing through them but not through the armatures, the armature voltages must be equally affected; and also see that the armature circuits are so arranged as to give the same net falling characteristics independent of the series coils. It is necessary for good compounding to have the brushes set once for all, and in all well-designed dynamos it is possible to find a point where there will be no excessive sparking. An easier although not so good a way to adjust is to find the best point for the lead and then alter the resistances of the series coils until all dynamos have the same net characteristics; but the rise of voltage due simply to the flow through all the series coils will in this case be different in different dynamos, and there will be a little more difficulty in adding a new dynamo, and a tendency to overload.

Nearly all of the compound dynamos in the market for the last few years are purposely built with too many turns in the series coil, and then a shunt of German silver is adjusted by trial at the factory until the voltage at the terminals shows the wished for "0 per cent. rise," or whatever it may be. Now for any one dynamo running under the conditions of the experimental trial this method is simple and perfectly correct; but when such dynamos are run together in multiple it is of no service, and has no effect on the relative actions of the series coils of the dynamos, because of the fact that all coils and all shunts become parallel conductors, and therefore traversed by currents inversely proportional to their resistances. These shunts then merely diminish the total flow of current through the series coils, but in no way do they serve to adjust the effect of the series coils relatively to each other. If it should happen that one dynamo was very much over-compounded and then shunted for the "0 per cent. rise," and another only very slightly over-compounded and consequently only slightly shunted, it is plain that when the shunted series coils are in parallel the shunts will have only the effect of affording a path for a part of the total flow of current, and consequently could be wired up in any convenient place on the switch board. These two above-mentioned dynamos would then be dissimilar over-compounded machines working in parallel, and in all probability an increasing load would be largely carried by the dynamo with most turns in the series coil, because an increase in total current flowing through the series coils would promote the voltage of this dynamo faster than that of the other. This will be the familiar condition we often see in power plants where the average load is equally divided, while an overload is taken quicker by the dynamo which takes the less at light loads, that is, the one in which the series coils have greatest effect.

In order to make such dynamos work properly it is best to examine the lead and the wiring, and, if possible, give the armatures similar net falling characteristics, and then, by the use of copper or German silver ribbon, slightly change the resistance of the series coils until all armatures are equally affected by the flow of current in the series coils; a single large shunt may be connected at any part of the bars to which the series coils are attached, or perhaps better each machine may have a shunt across the series connections at the switch board, on the dynamo side of the main three-pole switch, so that the total shunting capacity varies as it ought with the number of dynamos running.

After one of my lectures a few weeks ago two young men asked me for an explanation of what seemed to them a very strange phenomenon which always occurred when a large dynamo was cut out of service; and, as it is a very good practical example of the use of multiple characteristics as I have shown them, I will give it. In a railroad power house are about sixteen 100 horse power "0 per cent. rise" dynamos and one large 500 horse power "0 per cent. rise" dynamo, all provided with shunts. Ordinarily the machines all do well enough, but as the load gets light at night and the big one is cut out the voltage rises instantly about 30 volts in some unexplained manner. May not this be the reason. The series coil and shunt of the large dynamo are of so low resistance that they carry a large part of the total current in all the series coils and shunts. Now when the machine is to be cut out, the current in the armature is cut down by the rheostat in the shunt field circuit to as nearly as may be nothing; but this current is in this way slowly shifted to the other sixteen armatures, causing a slight fall in voltage due to the net falling armature characteristics, which drop is not counteracted by the series coils because there has been no change in the distribution of the total current. This slight fall is probably not especially noticeable, but the sudden jump comes when the series coils and shunt ribbon of the 500 horse power dynamo are cut out of circuit and the large current in them is instantly transferred to the series coils of the other smaller dynamos. If the switches were made to stand the arcing I doubt if there would be much change noticed if the dynamos were simply disconnected without gradually diminishing the armature current.

It has been supposed in this paper that there is no such a thing as time required to change the strength of dynamo magnets, and that there is no difference between the ascending and descending magnetizing curve of iron. These time effects will not come in for ordinary fluctuations if the net armature characteristics are similar, and consequently the required changes in all the magnets would be of the same amounts and would therefore take nearly the same time; for sudden changes like a short circuit it is not likely that the load would divide equally, but it would be very much nearer so than with the ordinary unadjusted dynamos, and a heavier short circuit could be burned out than with unadjusted dynamos, where some would rapidly overload and open their circuit breakers and thereby precipitate a general breakdown.

In large plants where more than one dynamo is driven by a single engine and there are several engines, it will be found of advantage to adjust each set of dynamos so that they will work well together, and then adjust each engine load for engine speed change. To do this, each dynamo on a single engine would have its series coil and shunt properly adjusted until, as far as that particular engine load is concerned, the load would be equally divided between the armatures ; then the sets of series coils corresponding to the different engines would be connected through a small resistance to the station bus bar, and we should find that the total load could be evenly divided between the engines. The arrangement of these resistances is indicated in Figure 7,

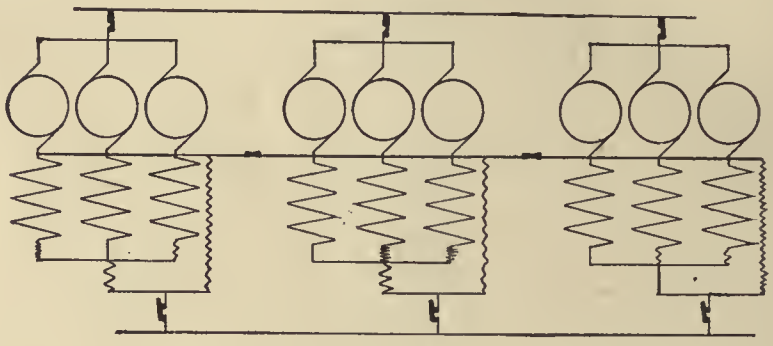


Fig. 7.

which shows three engines, each driving three dynamos by belts or any convenient way. The three series coils of each unit are adjusted so that a changing load will be equally shared by each dynamo, and then the three coils are shunted until the desired compounding is attained. It is now necessary to compensate for any difference there may be in the speed regulation of the engines or in the main belt slips by a slight change in the total resistance of the three sets of parallel series coils. There will be, of course, the usual precautions necessary when throwing in or cutting out a set of dynamos, and the most satisfactory way, if steady voltage is to be desired, will be to close the three large main switches at once, by steam or other power acting upon heavy switches built to stand the arcing of such currents, thus connecting simultaneously the three bus bars to the station main bars ; the same method should be used to disconnect when the time comes for stopping an engine.

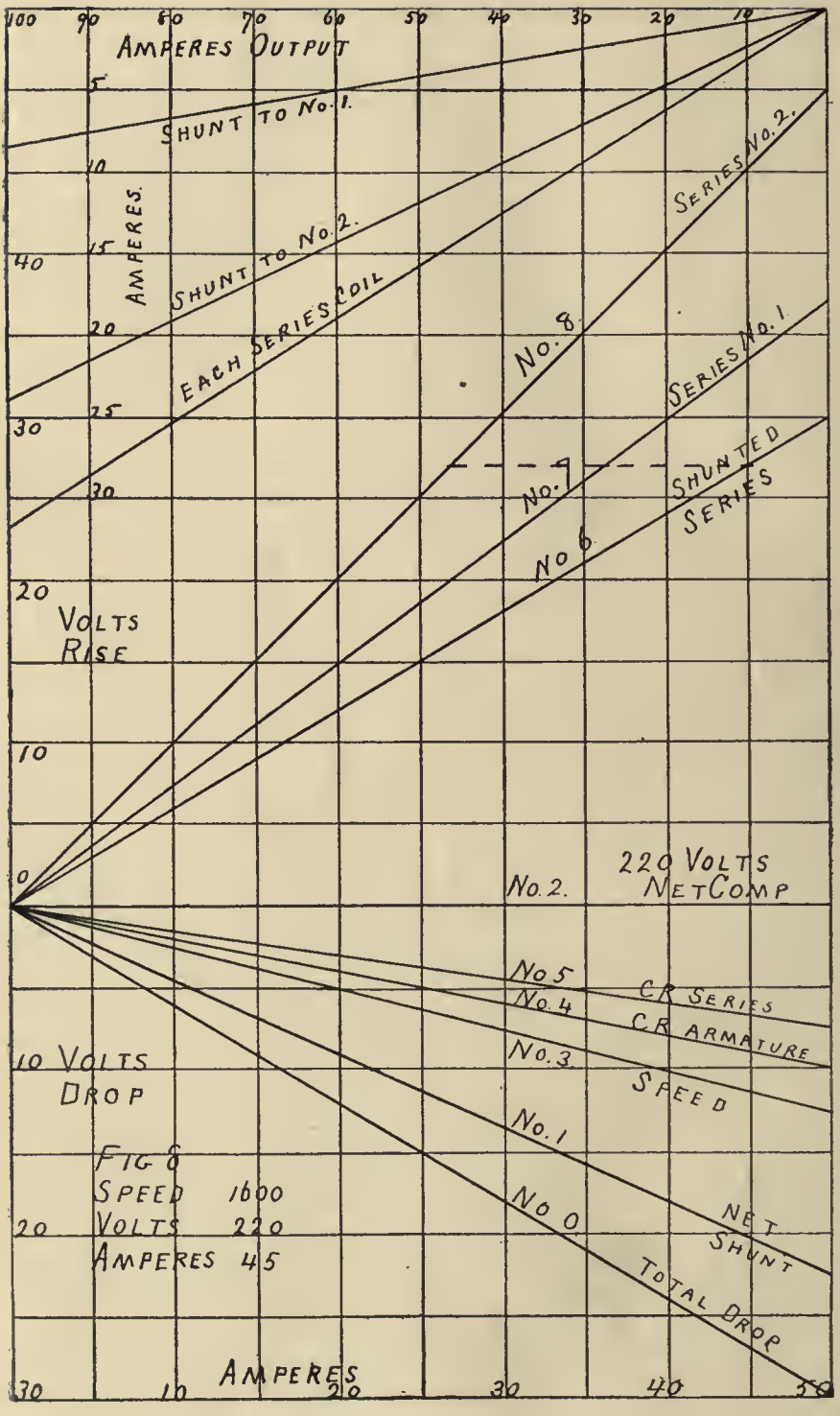
In order to study the action of dynamos which were exactly similar in all respects except the magnetizing effects of their series coils, a number of turns of wire was wound upon the magnet spools of dynamo No. 2 and connected in series with the regular windings, and the resistance of the series coils of dynamo No. 1 was made equal to that of No. 2 by the addition of a length of copper wire. Each dynamo was then fitted with the usual German silver shunt of such resistance that the net result was a constant potential at the terminals.

The various curves of these dynamos are plotted in Figure 8, and may be described as follows: Curve 3 gives the fall of potential or drop in volts due simply to the fall in speed as the load on the dynamo increases, and is the same for each separately driven dynamo. Curve 4 gives the drop in volts in the total armature circuit from equalizer wire, which will be considered as a bus bar, to main bus bar, and does not include the drop in the series coils and connection to the bus bar, which is given separately in Curve 5.

Curve 0 is the sum of Curves 3, 4, and 5, and represents the total drop in each dynamo, when running independently, due to all reactions; while Curve 1 is the sum of 3 and 4, or the total drop if we consider the series coil as a part of the external circuit. Curves 7 and 8 show respectively the rise in voltage due to the current in the series coils of each dynamo, and it will be seen that for the same current value, dynamo No. 2 has a somewhat more powerful series coil. Curve 6, being the equal and opposite of Curve 0, shows the rise in volts which each dynamo must have in order to maintain a constant potential at its terminals; and therefore a horizontal line cutting Curve 6, as, for example, at the rated current of 45 ampères, will cut the Curves 7 and 8 at points whose corresponding current values are those which will produce the required magnetizing force, and the difference between these current values and the rated current must be carried by the shunts on the series coils.

The series coils of each dynamo were adjusted to a resistance of .15 ohm, and the series coil shunt of dynamo No. 1 had a resistance of about .60 ohm and would carry at full load about 9 ampères, leaving 36 ampères to produce the rise of 27 volts necessary to neutralize the total drop taken from Curve 0. To produce the same rise in No. 2 dynamo required a shunt of about .22 ohm carrying 18 ampères, leaving but 27 for the series coils.

These two dynamos are now in such a condition that according to


100 90 80 70 60 50 40 30 20 10
AMPERES OUTPUT
5
10
15
20
25
30
AMPERES.
SHUNT TO No. 1.
SHUNT TO No. 2.
EACH SERIES COIL
SERIES No. 2.
No. 8
SERIES No. 1
No. 7
SHUNTED SERIES
No 6
40
30
20
10
0
VOLTS RISE
No. 2.
220 VOLTS NET COMP
No 5
No 4
No. 3.
No. 1
No 0
C.R SERIES
C.R ARMATURE
SPEED
NET SHUNT
TOTAL DROP
10 VOLTS DROP
FIG 6
SPEED 1600
VOLTS 220
AMPERES 45
20
30
10
AMPERES
20
30
40
50

the usual practice it would be perfectly proper to run them in parallel with the expectation that everything would work well; however, such is not the case, as may be seen from the curves and as was proved by the actual effects obtained.

When the two series coils with their shunts are thrown in parallel the total flow of current through them will divide inversely as the resistances of the four circuits which are, as we have just seen, two series coils each of .15 ohm, one shunt of .60 ohm and one of .22 ohm. If we call the total current 100 these four circuits will carry respectively about 33.7, 33.7, 8.4, and 24.1, and as the total output of the two armatures may safely be 100 ampères, the above figures give the strength of current in these circuits.

At the top of Figure 8 is a scale reading from right to left up to 100 ampères, and near the left hand side a scale reading downwards up to 30 ampères; the three inclined lines enable us to read at once the actual current in the two shunts and the two series coils for any output of the dynamos when running in parallel.

It has already been shown that each dynamo is individually capable of maintaining a constant potential at the bus bars independently of the current flowing, and it has been stated that the dynamos will not work well in parallel. Suppose that the dynamos are connected in parallel at 220 volts under no load and that a current of 90 ampères, which is equal to the rated load of the two dynamos, is taken from the bus bars To find the currents in the series coils we find under 90 ampères total output that each series coil will carry 30.4 ampères; the shunt on No. 1, 7.6 ampères, and the shunt on No. 2, 21.6 ampères.

No. 1.	No. 2.	
220.	220.	Volts at no load.
+ 22.5	+ 30.2	Rise from Curves 7 and 8 at 30.4 ampères.
242.5	250.2	Total armature voltage.
	— 7.7	Fall of volts due to 17 ampères, Curve 1. No. 2 can deliver current producing a fall of 250.2 — 242.5 before No. 1 begins to act as a dynamo at all, and after that both armatures will divide the remainder, or 90 — 17 = 73, and the total drop is taken from Curve 1 for one half the difference or 36.5 ampères.
242.5	242.5	
— 16.5	— 16.5	Drop with 36.5 ampères, Curve 1.
226.0	226.0	Armature bus bar voltage.
— 6.5	— 6.5	Drop in the series coil giving for the main bus bars 220.5 volts with an output of 36.5 ampères from dynamo No. 1, and 53.5 from No. 2.
220.5	220.5	

As an interesting example of the different degree of badness which may be produced when two individually well-adjusted dynamos are run in parallel, let us suppose that these two dynamos are placed in proper allignment and one large driving pulley slipped over the ends of the shafts, as is very often done, thus making the armatures run at the same speed with the same per cent. belt slip, which will bring the resultant voltage about the same as in the previous example.

No. 1	No. 2.	
220.	220.	Volts at no load.
22.5	+ 30.2	Rise from Curves 7 and 8 as before.
242.5	250.2	Total armature voltage.
	— 7.7	Fall of volt due to 38 ampères. Curve 4.
242.5	242.5	Dynamo No. 2 must deliver a current producing a fall of volts equal to the difference of the armature voltages before No. 1 will deliver any current. The remaining current will divide equally, producing a fall found in Curve 4.
— 5.2	— 5.2	Fall with 26 ampères, Curve 4.
237.3	237.3	
— 6.5	— 6.5	Drop in series coils. Curve 5.
230.8	230.8	
— 11.2	— 11.2	Belt slip. Curve 3.
219.6	219.6	Voltage on main bus bars with an output of 26 ampères from dynamo No. 1 and 64 from No. 2.

COMPARATIVE TABLE.

	No. 1. Ampères.	No. 2. Ampères.	Volts.
Separate loads	45.	45.	220.
Parallel	36.5	53.5	220.5
Parallel at same speed	26.	64.	219.6

BOWLDERS FORMED IN SITU.

By GEORGE H. BARTON, S.B.

Received April 15, 1893.

The neighborhood of Boston furnishes many localities exceptionally well adapted for instruction in the field of classes in geology. Within a few minutes' to an hour's ride from Boston the student can reach many places, as, for instance, Roxbury, Somerville, Marblehead, and Nantasket, where illustrations of the various features of structural geology abound. The object of this paper is to call attention to a locality of exceptional interest for this latitude, exhibiting the formation of bowlders *in situ*.

It is well known that beyond the limits of the glaciated areas the surface rocks are decomposed to a considerable depth while they still retain their normal stratigraphic relations, and that veins of less decomposable material like quartz preserve their original position unchanged.

In some places where the elements of erosion have acted more rapidly than the process of decomposition, fragments of the original mass, more or less rounded, from which the decomposed portions have been washed away, are left upon the surface as bowlders.

Dr. T. Sterry Hunt, in a paper upon the "Decay of Crystalline Rocks,"[1] calls attention to the principal papers published upon this topic, and discusses at length the subject of decomposition. He arrives at the conclusion that the change took place when the atmosphere was in a highly carbonated condition and a climate very different from our present one prevailed.

L. S. Burbank[2] has emphasized strongly the formation of bowlders *in situ* as studied by him among the foothills of the Blue Ridge in North Carolina.

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[1] Mineral Physiology and Physiography, pp. 246–278.

[2] Proceedings of the Boston Society of Natural History, November 19, 1873.

Prof. C. F. Hart, in his work on the "Geology and Physical Geography of Brazil," and many others, might be cited on this subject.

Passing over the divide to the eastward of Butte, Montana, the writer noticed a large area of granitic rock, highly weathered, from which in many places the loose materials had been swept away, leaving subangular and rounded blocks scattered over the surface closely resembling the accumulations of the same forms by ice-action. Thus far reference has been made only to non-glaciated regions. Within the limits of the area formerly coated by the great continental ice-sheet, where nearly all the previously loosened material suffered transportation by its agency, accumulations of decomposed material *in situ* are comparatively rare.

When such accumulations are found, careful observation is necessary to determine whether these are remnants that were undisturbed by the action of the ice-sheet, or are the result of decomposition during post-glacial times. The place referred to in this paper is near the center of Medford, Massachusetts, north of the street leading to West Medford, and along Brook's Lane. The exposure consists of a part of the dike, which, outcropping at Granite Street and also near the Old Powder House in Somerville, continues north through Medford nearly to Spot Pond, Stoneham.

This dike has been referred to by nearly every writer upon the geology of Eastern Massachusetts, but has been most fully described by Dr. W. H. Hobbs.[1] This author describes the rock as essentially a diabase, "composed mainly of a plagioclase feldspar and an amphiboloid mineral with more or less biotite and pyrite." The rapid decomposition of the rock is due to "the amphiboloid mineral, causing a complete disintegration of the rock to a coarse feldspathic sand." "The products of alteration of the feldspar are calcite and a mineral which is probably kaolin."

The present recent exposures are immediately on the west side of Brook's Lane, and have been caused by excavations in connection with the building of new streets and grading around the new club house just erected. On the east side of a north-south ridge about forty feet high the dike rock is exposed in vertical section for many rods in length and varying in height from a few inches to twenty feet.

Throughout this entire section the rock is entirely decomposed on the vertical surface so that it readily crumbles in the fingers, though

[1] Bulletin of the Museum of Comparative Zoölogy, 16 (Geol. Series 2), 1–12.

occasionally a few patches of comparatively unchanged rock are visible, surrounded by the concentric structure common in decomposing diabase.

Occasionally, also, undecomposed veins still retain their original position, passing through the decomposed rock as shown near the extreme left of Figure 1, and also on the extreme right. Here, also, they are seen alike passing through the undecomposed fragment still *in situ* and the completely decomposed material above.

Fig. 1.

Along the larger portion of the exposure the looser decayed rock has fallen down, leaving exposed the masses still unchanged, or only slightly changed, many of which remain in place projecting from the bank, while others have fallen down and accumulated along the bottom. These latter appear very similar in form to the rounded and subangular glacial bowlders that are scattered over the surrounding country.

Fig. 2.

Figure 2 shows these characters very finely. The entire section shown in this, as in all three views with one exception to be noted, consists of the decomposed rock inclosing the unaltered fragments projecting from the mass and a row of the fallen fragments at the base.

The process of the formation of bowlder forms is shown in the mass above and to the left of the hand bag where two medium sized fragments, with a small V-shaped mass between, still retain their original position. The lines between these three portions show where decomposition has gone on along the joint cracks and the decayed material has been washed away. The same process is shown, though less distinctly, in the large mass a couple of yards to the right.

Fig. 3.

In Figure 3 the incipient breaking up of one large mass into four smaller ones is shown just to the left of the center. This view is of especial interest, however, as showing unmodified drift overlying a portion of the decayed rock. The contact can be easily traced as a nearly horizontal line from the extreme left, just above the quartered mass mentioned above, to a point just to the right of the center, where it suddenly changes and is continued as a vertical line to the surface. There is not sufficient evidence seen to indicate whether this is due to faulting or not. The unmodified drift is easily distinguished by the numerous small subangular fragments inclosed which are entirely unchanged.

In a few cases glacial striæ are to be seen crossing surfaces of unchanged rock, and also of the decomposed rock in immediate contact, in such a manner as to indicate that the now completely decomposed rock was unchanged when the striæ were formed. This would lead to the conclusion that the decomposition has taken place during postglacial time. This view is strengthened by the fact that portions of the rock exposed as outcrops have remained unchanged, while immediately adjacent portions which have been covered by the soil are highly decomposed. This last feature indicates that organic action is a strong factor in the production of decomposition.

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